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## Experimental, Theoretical Clinical and Historical Psychology

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# AN ATTEMPT AT BUILDING A THEORY OF CONDITIONED REFLEXES ON SPINAL REFLEXES*

*From the Psychological Laboratories of the Gosudarstvennei Institut Eksperimentalnoi Psikhologii*

V. Borovski

## 1. What Facts from the Study of Spinal Reflexes Shall We Have to Make Use Of?

When we observe in the final common path a spinal reflex replacing another, preceding reflex which formerly held the final common path, we call this new reflex prepotent. We assume that the less potent reflex is suppressed by the prepotent. But the conception of the potency of a reflex does not add anything to the picture actually observed. When we speak of the prepotency of a given reflex, all we mean to say is that actually it suppresses another reflex. We may speak of the *prepotency of a stimulus,* if it evokes a prepotent reflex. According to Sherrington, in the competition of reflexes in the final common path, the issue depends mainly on three kinds of factors:

1) *On the intensity of the stimulus.* The greater the intensity of the stimulus, and the nearer the point of application to the focus of the receptive field, *the more potent* is the reflex evoked.

2) *On the functional specificity of the reflex.* Some reflexes, whatever the intensity of the stimulus, always suppress the others. We may speak of the specific potency of certain reflexes. All reflexes may be placed in a certain scale according to their specific potency. The upper end of the scale will be taken up, generally speaking, by reflexes produced by noxious stimuli, whereas the lower end will be occupied by postural reflexes which, as we know, are easiest interrupted by all the others. Biologically such interrelations are, of course, quite comprehensible. Food reflexes will stand higher or lower, depending upon the degree of hunger; sexual reflexes will depend upon the season; and all, in general, will depend upon the physiological status of the organism. The food reflex, for example, will sometimes set aside the reflex to a noxious stimulus; if the animal is very hungry, food will be prepotent, and the animal will

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fight for it at the risk of being wounded; but, if the wound received is deep, the animal will give up the attempt to obtain the food, i. e., the noxious stimulus will prove to be more potent than the food.

3) *On the fatigue of the final common path with relation to the given reflex.* If a reflex has made use of the final common path for a long time, its potency gradually declines, and another reflex, formerly less potent, now suppresses it. Here we are probably dealing with a case of a more common biological law, which is, namely, that any reaction of the animal not leading to a biologically valuable effect will by degrees be produced less and less energetically, and may finally become extinct.

An example of this is the pike, which, being unable to get hold of the minnows placed behind a glass partition, ceases knocking its head against it, and, even after the partition is taken away, will no longer attack the minnows.

We may formulate this as follows: If a reaction to a given stimulus, under given conditions, does not lead to a biologically valuable end, the potency of the stimulus, when repeated, becomes less and may become null.

## II. Definition of the Conditioned Reflex

We speak of a conditioned reflex to a given stimulus in the case when the animal, in its individual life, begins to display not the reflex which it has performed from its birth but another, namely, one it has performed before to another stimulus. The latter condition is very important. If this is not observed, the new reflex to a given stimulus may be elicited by physiological growth, development of new organs, for example, sexual and the like. In that case there would also be a change in the innate connection between stimulus and reflex, but nothing like what we call the conditioned reflex. We repeat that in the conditioned reflex we see that a reflex performed by the animal earlier begins to appear in response to a different stimulus. More exactly we call such a phenomenon a *positive* conditioned reflex.

It is evident, then, that we wish to contrast it with the *negative* conditioned reflex. By this term we understand the elimination, acquired during the individual life of the animal, of the innate connection between the stimulus and the reflex elicited. In other words, we speak of a negative conditioned reflex when we observe that a given stimulus ceases to elicit the reflex which it primarily elicited, and that such a change in behavior is acquired, i. e., it does not depend on the atrophy of any of the organs, on senility, or the like. In a more complicated case, when we are dealing with the reactions of the organism as a whole, the negative conditioned reaction

coincides with what Hamilton[1] calls *apparent unresponsiveness,* and what I, in one of my previous works,[2] called conditioned indifference.

To be more exact, such positive and negative conditioned reflexes as we have just defined, we specify as conditioned reflexes of the *first order.* If, however, the conditioned reflex is not built upon the unconditioned but on a previously established conditioned reflex, we shall have reflexes of a second, or a higher order.

## III. Rules for Obtaining Conditioned Reflexes

From all the observations and experiments that we have at present on the production of conditioned reflexes, we may deduce two empiric rules or laws, into which the facts known to us seem to fit in well.

The first rule, or the *time-rule,* says that the future conditioned reflex should somewhat precede the unconditioned. This rule, therefore, sets up a certain sequence in time between both stimuli. For example, if we wish the light of the lamp *L* to become the conditioned stimulus for salivation, which till then was caused by the sight of meat *C,* we should first present *L,* following it (in a few seconds after) with *C.* After several repetitions *L* will begin to elicit the reflex *Rc,* which was formerly elicited by *C,* even without presenting the unconditioned stimulus *C.*

Let us take the stimuli in reversed order: we will present first *C,* and then *L.* Generally speaking, we shall not, in this way, make *L* the conditioned stimulus for *Rc.* It is true the negative result will not always be clearly expressed. This is quite comprehensible. For it is well known that, after the stimulation ceases, there is a more or less lengthy after-discharge. With *C* may be connected such an after-effect, which lasts longer than the interval between *C* and *L;* in that case we may suppose that the interval between *C* and *L,* plus some seconds more, is irrelevant in the formation of the conditioned reflex. Further, the after-effect from *C* elicits the usual reflex on *C* of *Rc,* which consequently now occurs after *L,* so that, in fact, the time-rule will be observed. The conditioned reflex will be obtained though we have applied the stimuli in the incorrect sequence. The exception to the rule may be only seeming.

The experiment just described brings about a whole number of consequences. Indeed, we have just applied *C,* then *L.* There is no doubt that *L,* in itself, elicits a certain unconditioned reflex *Rl;* it may be the dog will half close its eyes, or put its tail between its legs, or the like. The question is, then, why does not *C,* according

[1]Hamilton, G. V. An introduction to objective psychopathology. St. Louis: Mosby, 1925.

[2]Borovski, V. M. Experimentelle Untersuchungen über den Lernprocess. *Zsch. f. vergl. Physiol.,* 1927, **6**, 489-529.

to the first rule, become the conditioned stimulus for *Rl*? It is evident that mere correlation in time is not sufficient, as it is not at all clear to us why, of the two stimuli *C* and *L*, normally *L* becomes the conditioned stimulus for *Rc*, and not *C* for *Rl*. This brings us to the necessity of setting up some other, second rule.

The second rule we call the *prepotency rule*. This rule determines the necessity of a definite relation between both stimuli according to their potency; namely, the unconditioned stimulus should be of greater potency than the future conditioned stimulus. In fact this rule describes, only in other terms, what we are trying to attain. For what we want is that *Rc* and not *Rl* should respond to *L*, i.e., that *Rc* should suppress *Rl;* and this is just what we have called the prepotency of *Rc*, compared with *Rl*. And so *C* should be more potent than *L*.

This rule, however, may also become complicated by accidental circumstances. For example, let us imagine *L* as being more potent than *C*. But we know that frequent repetition, as well as long-maintained excitation of one and the same stimulus, may call forth fatigue of the final common path in relation to the given stimulus. Repetitions may reduce the potency of *L;* the conditions necessary to the second rule will be present; the reflex will take longer to form; a greater number of repetitions will be needed, but nevertheless it will be obtained. The exception, again, will be only seeming.

Here is an example from experiments of the Pavlov school, agreeing exactly with the above-said. Pavlov says: ". . . . in the sphere of acoustic phenomena attention was drawn to the fact that very strong excitations, forcible ringing of bells, did not very soon become conditioned stimuli of the saliva glands, when compared with weaker sounds. It must be supposed that the strong acoustic stimuli themselves call out considerable (motor) reactions in the organism, and that these reactions retard the production of saliva reaction." The rule of prepotency, together with the occurrence of fatigue, wholly embraces the facts described by Pavlov.

Up till now we have somewhat simplified the facts concerning the formation of a conditioned reflex. In reality *Rc*, in most cases, does not completely set aside *Rl*, for probably their final paths are not all common. It is true that *Rl*, not having under the given conditions direct biological value, wanes in consequence of fatigue. (If it had biological value, its potency would grow with repetition, and we should not get a conditioned reflex.) Still, generally speaking, there are left from *Rl* some remains—*Rl'*. Then, by presenting *L* we shall finally have the following picture: *L* is followed by *Rl'*, and this in turn by *Rc*—or rather not *Rc* but $(Rl'+Rc)$; but

since the latter should not be looked upon as an algebraic sum, it is therefore preferable that the final result should be denoted as *Rl'c.*

The original *Rl,* after *C* was presented, coincided in time with *Rc.* If the interval between *L* and *C* had been so long that *Rl* would have been ended before the appearance of *C,* we should not have obtained the conditioned reflex. Both reactions *Rl* and *Rc* occurred in the organisms simultaneously. Or, we should say rather that the organism produced a kind of reaction in which *Rl* and *Rc* were somehow interconnected. Further, as we see, *Rc* partially suppresses *Rl.* There is nothing wonderful in the fact that the influence upon one another grew more and more intimate. So we can very well imagine that the remains *Rl',* which now directly pass over to *Rl'c* have acquired the character of a stimulus for *Rl'c;* so that now *L* elicits *Rl',* and *Rl'* immediately evokes *Rl'c. Rl'* is *that very link which creates the conditioned reflex* which was lacking and was necessary for it to appear; as soon as *Rl'* begins to call forth *Rl'c,* we have before us the conditioned reflex.

## IV. The Combination of Positive Conditioned Reflex with the Negative

Let us turn back to Pavlov's experiment with strong acoustic stimuli. We see that the reactions produced to them were compelled, in time, to decline in order to make it possible for a conditioned reflex to them to be produced. At the extreme, the stimulus *L* would not produce the reaction *Rl,* which it originally had called out. In other words, here a negative conditioned reflex to *L* was produced. We may say that such a component is always existent in the process of the formation of a conditioned reflex; together with (or somewhat previous to) the formation of a positive conditioned reflex we produce another, a negative reflex.

Just as we eliminate the innate reflex, so can we, of course, also eliminate a previously formed positive conditioned reflex. In other words, we can produce a negative conditioned reflex, opposed to the positive reflex, which we wish to set aside. We could remove the latter even with the help of a third, still more potent reflex, i.e., we could take a new stimulus *M,* more potent than *C,* and suppress *Rl'c* through *Rm.* But we can also act otherwise, that is, after *Rl'c* has responded to *L,* we can systematically *not apply C;* then *Rc* will not have any biological meaning, and will gradually disappear, just as it was with the reactions to loud sounds. *Rc* will not appear any more. *L* will cease to elicit *Rl'c.* The positive conditioned reflex will be eliminated; it will be removed by our *systematically building* the opposite negative reflex, just as we previously systemati-

cally produced the positive. For example, we obtained salivation to lamplight; now we shall repeatedly apply the lamplight but shall not give the meat after it. Then the salivation to the lamplight will be of no biological value for the dog, and will gradually cease.

Suppose we have first formed a conditioned reflex, and then an opposite negative one. After a lengthy pause we again apply *L*. Then *Rl'c* may appear again. This will indicate that we have produced a negative conditioned reflex less resistant than the positive.

## V. More Complex Combinations of Conditioned Reflexes among Themselves and with Unconditioned Reflexes

First of all, let us try to analyze schematically one comparatively simple case. Suppose we have obtained a conditioned reflex to stimulus *L*. Now we will introduce, for the first time, a new stimulus *N* simultaneously with *L*, or immediately after it. Here we may have the following combinations:

1) *The difference between N and L is above the differential threshold.*

*a*) *Rn* allied to *Rl*.

(1) The positive *Rl'c* will be enhanced, or *N* will elicit the positive *Rl'c*, though we have not formed any positive reflex to *N*. *Example*: We have a resistant *Rl'c* to sound; the experimenter proves that *Rl'c* is present; directly applies *C;* then, after a pause of ten or fifteen minutes, applies another acoustic stimulus *N* (bell, whistle, gramophone, or the like); *N* elicits a conditioned reflex though it has never before been combined with *C*. Visual or olfactory *N*-stimuli also produce an effect, but weaker.

(2) If a negative conditioned reflex has been formed to *L*, that is, if *Rl* is not produced to *L*, then *N* may call forth the appearance of *Rl*, as *Rn* will enhance the remains *Rl'*.

*b*) *Rn* antagonistic to *Rl*.

Being sufficiently potent, *Rn* may completely suppress *Rl'c*. *Example*: While the dog is on the floor, the showing of dry bread calls forth salivation; but if the dog were, for the first time, put on the table (a new *N*), then *L* does not produce the least effect. Such a phenomenon Pavlov calls external inhibition.

2) *The difference between N and L is below the differential threshold.* *N* applied without *L*, instead of *L*, will elicit the same conditioned reflex which was obtained to *L*. This case is sometimes denoted as generality of reflexes.

3) *The difference between N and L lies near the threshold.* In this intermediate case *Rn* may have features of similarity and features of antagonism. If the difference at first checks *Rn*, but its antagonism has no immediate biological value; when it is repeated the antago-

nistic features will wane. Then the features of similarity will appear, and the case 1*b* will become case 2, i.e., *N* will itself begin to elicit *Rc*. Examples of this exist in the experiments of the Pavlov school.

On the basis of such considerations we can approach even the more complex phenomena. For instance, let us take the so-called delayed reflex. As is known, it is obtained in such a way that at first the ordinary *Rl'c* to *L* is established, and then the gradual lengthening of the interval between *L* and *C* is commenced, bringing it, for example, to three minutes. Then *Rl'c* ceases being produced in half a minute, in one minute, in two minutes, and so on, and at last will be produced only in three minutes. In other words, we obtain a number of negative conditioned reflexes to *L* in one-half, one, two . . . minutes, and one positive *Rl'c* to the same *L* in three minutes. If during the interval, a certain *N* is applied then *Rl'c* may appear prematurely. We can explain this by the fact that *Rn* is allied to *Rl*, and, therefore, (just as in the case 1*a*) the former eliminates the negative conditioned reflex.

Evidently the combination of conditioned reflexes among themselves and with the unconditioned occurs upon the same principles as the combination of spinal reflexes. Just as two scratch-reflexes are allied when they are called out from near spots, but inhibit one another when the stimulated spots are taken sufficiently apart, so can conditioned reflexes enhance or inhibit one another.

Conditioned reflexes may prove to be allied or antagonists to other conditioned and unconditioned reflexes. Further, the matter becomes complicated also by the fact that the conditioned reflexes may be obtained not only to an existing stimulus but to an after-effect. Here also the occurrence of induction plays a great rôle. Thus the most varied combinations may occur, but even the most complicated of them allow of such an analysis as we have carried out above, and may be understood on the basis of the facts, which have been established in relation to spinal reflexes.

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## ESSAI DE DÉVELOPPER UNE THÉORIE DE RÉFLEXES CONDITIONNÉS DES RÉFLEXES SPINAUX

(Résumé)

Un réflexe a une plus grande force quand il fait disparaître ses antagonistes. Sur cette théorie on base deux règles empiriques. La *règle de temps* indique que le stimulus conditionnel futur doit être appliqué peu de temps avant le stimulus inconditionné. La *règle de force antérieure* indique que le

stimulus inconditionné doit avoir une plus grande force que le stimulus inconditionnel futur.

On peut expliquer ainsi le développement d'un réflexe conditionné: Le réflexe inconditionné produit d'abord par le réflexe conditionnel futur est plus tard supplanté par le réflexe même causé d'abord par le réflexe inconditionné. D'ailleurs, le premier réflexe n'est pas entièrement supplanté par le second. Il reste un certain stimulus, évidemment un facteur important. Car, puisqu'il court dans l'organisme simultanément avec le second, il doit se confondre avec celui-ci dans une unité qui représente le réflexe conditionné désiré. Le complétement du réflexe conditionné se trouve au même moment où ce stimulus restant, mentionné là-haut, atteint la *valeur d'un stimulus* pour la production de son compagnon, c'est-à-dire, le réflexe pour le stimulus inconditionné.

Si l'on peut causer (développer) que dans le cours de la vie individuelle de l'organisme, un certain stimulus cesse de produire le réflexe causé d'abord, on l'appelle alors un réflexe conditionné négatif—faute de mieux. On peut considérer tous les faits concernant les réflexes conditionnés jusqu'ici établis, comme des combinaisons, plus ou moins compliquées, de réflexes inconditionnés positifs ou de réflexes conditionnés négatifs. Ces combinaisons suivent exactement les principes établis pour les réflexes spinaux. De la même façon, se trouvent ici des réflexes alliés et antagonistes. Ici, de même, les effets ultérieurs et les soi-disant inductions sont très prononcés. Au moyen de telles conceptions, établies d'abord pour les réflexes spinaux, on pourra probablement expliquer tous les faits décrits, par exemple, par l'école de Pavlov. BOROVSKI

## EIN VERSUCH EINE THEORIE DER BEDINGTEN REFLEXE AUS SPINALEN REFLEXEN ABZULEITEN

(Referat)

Ein Reflex besitzt grössere Potenz wenn er seine Antagonisten verdrängt. Darauf begründen wir zwei empirische Regeln. Die *Zeitregel* besagt, dass der zukünftige bedingende Reiz kurz vor dem unbedingten angewandt werden muss. Die *Praepotenzregel*—dass der unbedingte Reiz grössere Potenz besitzen muss, als der zukünftig bedingende.

Der Prozess der Ausbildung eines bedingten Reflexes besteht darin, dass der unbedingte Reflex, welcher anfangs durch den zukünftigen bedingenden Reiz ausgelöst wurde, späterhin durch denjenigen Reflex verdrängt wird, den der unbedingte Reiz hervorruft. Es wird jedoch der erste Reflex durch den zweiten nicht völlig verdrängt. Es bleibt ein gewisser Rest bestehen, dem wir eine grosse Rolle zuschreiben müssen. Da er nähmlich gleichzeitig mit dem zweiten Reflexe im Organismus verläuft, so muss er mit ihm zu einem Ganzen verschmelzen. Dieses Ganze wird alsdann den gewünschten bedingten Reflex darstellen. Die Ausbildung des bedingten Reflexes wird in demselben Momente erreicht sein, wo der erwähnte Rest den *Wert eines Reizes* für die Auslösung seines Gefährten, d.h. des Reflexes auf den unbedingten Reiz, gewinnen wird.

Wenn wir es erreichen (ausbilden) können, dass im Laufe des individuellen Lebens des Organismus ein gegebener Reiz aufhört denjenigen Reflex auszulösen, den er ursprünglich ausgelöst hatte, so sprechen wir, in Ermangelung einer besseren Bezeichnung, von einem *negativen* bedingten Reflexe. Alle die bis jetzt über bedingte Reflexe festgestellten Tatsachen können nun als mehr oder weniger komplizierte Verknüpfungen unbedingter, positiver

und negativer bedingter Reflexe aufgefasst werden. Und zwar gehen diese Verknüpfungen nach genau denselben Prinzipien von statten, die für spinale Reflexe aufgestellt worden sind. Ganz ebenso finden wir auch hier alliierte und antagonistische Reflexe. Auch hier spielen Nachwirkungen und sogenannte Induktionen eine grosse Rolle. Mit Hilfe solcher ursprünglich für die spinalen Reflexe aufgestellten Vorstellungen können wahrscheinlich alle Tatsachen erklärt werden, welche z.B. von der Pavlov'schen Schule beschrieben worden sind.

Borovski

## ПОПЫТКА ПОСТРОИТЬ ТЕОРИЮ УСЛОВНЫХ РЕФЛЕКСОВ НА СПИННО-МОЗГОВЫХ РЕФЛЕКСАХ.

(Реферат).

Основываясь на том, что рефлекс имеет большую мощность при вытеснении им антагониста, мы выводим два эмпирических правила: *Правило предварения* гласит, что будущий условный раздражитель должен несколько предшествовать безусловному. *Правило большей мощности*—что безусловный раздражитель должен обладать большей мощностью, чем будущий условный. Процесс образования условного рефлекса состоит в том, что безусловный рефлекс, который первоначально вызывался будущим условным раздражителем, впоследствии вытесняется рефлексом на безусловный раздражитель. Все же от первого рефлекса сохраняется некий остаток, который производится организмом одновременно с рефлексом на безусловный раздражитель и спаивается с ним в один сложный рефлекс, который и является условным рефлексом. Условный рефлекс имеется с того момента, как упомянутый остаток приобретает *ценность раздражителя* для своего спутника, т.-е. для рефлекса на безусловный раздражитель.

Мы называем *отрицательным* условным рефлексом (за неимением лучшего термина) явление индивидуально выработанного устранения безусловного рефлекса на вызывавший его прирожденным образом раздражитель.

Все факты, установленные относительно условных рефлексов, можно понимать как более или менее сложные сочетания безусловных, положительных и отрицательных, условных рефлексов. Сочетания их происходят по тем же принципам, как и сочетания спинно-мозговых рефлексов. Рефлексы могут быть антагонистами или союзными. Играют роль явления последствия и индукции. На основе таких представлений, созданных первоначально для спинно-мозговых рефлексов, повидимому, возможно истолковать все факты, описанные, например, школой Павлова.

**Боровский (Borovski).**

# A CASE OF SYNAESTHESIA*

*From the Psychological Laboratories of the University of Edinburgh*

MARY COLLINS

The majority of reports on synaesthetic individuals are descriptive in character. The only warrant for adding another to the list is because of the extensiveness of the color associations of the case to be here described.

*S* has exceptionally strong imagery in all sense departments. She can image a sound, a taste, a smell, as vividly as if the actual sensations were experienced. In fact, sometimes the images become so intense that they give rise to hallucinations. *S* is able to answer affirmatively all the questions in Galton's questionnaire on imagery in his *Enquiries into Human Faculty*. Her brother can do likewise. She also has the power of eidetic imagery. Daydreams in childhood were mostly auditory, and, at the age of six, *S* used to send herself to sleep by telling herself stories aloud. Now her daydreams are composed of vision, touch, and hearing, and have a great deal of movement. *S*'s mother has very vivid mental images, and visions and hallucinations are very common in her father's family, but neither parent associates sounds or ideas with color as *S* does, nor does her brother possess this characteristic.

Color plays a very important part in *S*'s life, and she is always conscious of a colored background. She is never without this color background, which is part of herself, and she finds it difficult to imagine what other people's minds can be like devoid of this color. The color does not remain constant, but varies in regular manner according to the person spoken to, the conversation listened to, the music heard, or according to the task in which *S* is engaged. In listening to a lecture, for example, the topic of the lecture evokes a definite color; this color will form the background as long as *S* is interested in the lecture. If *S* changes her attention from lecture to lecturer, the color of the lecturer predominates. Different subjects call up differently colored backgrounds, and different persons cause different color to appear in the mind of *S*. For example, Chemistry is green, and while she is listening to lectures on Chemistry, green is

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the pervading color atmosphere. Anatomy is red, Ethics white, Metaphysics a nice grey; Psychology is white, but subjects in Psychology may have their own colors: Psychology of Suggestion would be brown.

People also have their own typical color and often scent attached to them. Individuals of no interest to *S*, or not well known to *S*, or of unimpressive personality, have no color. The colors of individuals are liable to change as *S* becomes better acquainted with them. Individuals may also become identified with flowers. Houses of friends all become associated with characteristic smells, which *S* finds impossible to analyze.

Books and authors are colored. *A Tale of Two Cities* is green; *Ben Hur* is fawn; Woodworth's *Psychology* is purple, probably because of the color of the cover in the British edition; Scott is grey; Masefield's poems are blue; Yeat's poems are bluish grey or greenish grey; Shaw is red. If *S*'s mental background is blue, then she must read appropriate books or poems or play appropriate music, that is, of the same color as her background. All music is colored for *S*, and if in listening to music there is no color, *S* does not enjoy it, which means that the music is too intricate for her to follow. The color in this case pervades the whole room or concert hall, and the natural colors around are subjectively repressed. Hebridean music is sea-blue. McDowall's music is red and is not stimulating to *S* but rather irritating. Bach and Beethoven are brown (*S*'s own color) and are most stimulating. The *Pathétique Sonata* is mostly brown but is green in one bit and yellow in the little humorous bars which end the main theme. *S* feels that Beethoven has felt the whole thing heavy and uses his sense of humor there. The color accompanying the music of the *Bluebird* (mainly Chopin's Waltzes) appears most appropriate—pale blues and greens. Grieg's music is colored green. His *Wedding* and *Processional* are "exactly the same green as the lamp-shade in the Psychology classroom with no light inside." Chaminade's pieces are "exactly the same green as the lamp-shade in the Psychology classroom with the light inside"—an interesting confirmation of the nicety with which colors must be described to be satisfactory for the colored thinker.

One octave up from middle C is blue, one octave up from G is green, one up from F is pink. Songs listened to are all colored, and remain the same color no matter who the singer may be. *S* always hears the song as it should be sung. A discordant note on the piano or in a song is colored magenta, a color to which *S* has a strong dislike. This color we shall see is associated with all things disagreeable, and the dislike of it seems to be inherited, as *S*'s father

also has a strong dislike to it. *S*, however, was unaware of this until lately.

Countries also have their own color. France is pink, Belgium yellow, Germany black, Scotland red, with tartan always in the background, England white, Ireland green, America strawish yellow.

Color associations have always been present in *S*'s mind, and she is always conscious of having experienced them. It is evident, however, that new associations are constantly being formed as her knowledge increases and her interests grow wider. There is undoubtedly a continual growth in the color associations. There is no intention of suggesting that there is deliberate searching for a color, for that is not the case. The color comes quite spontaneously, but if *S* is asked off-hand the color of such and such a thing, with which perhaps no color is yet associated, she may have to think about it, but she can very readily reject a color suggested by a second person as being quite incongruous.

If we now systematize our data and group them under the various sense departments, we get the following results:

*Taste*:

All tastes are colored, and colored very vividly.

Sweet tastes are pink.

Acid tastes are green.

Salt tastes are blue.

Bitter tastes are magenta.

Curry tastes yellow (evidently due to original color); orange also tastes yellow, but a different shade (again due to color of object). This taste photism exists in anything tasting of orange, no matter what its color, and so with the others.

*Odors*:

Horrible sharp odors are magenta.

An unpleasant odor like putrefaction is brown.

Pleasant odors vary, and the colors they evoke are much more difficult to explain.

Lavender is colored pale straw yellow, and not lavender as one might suppose. (*S* thinks this may be due to Yardley's yellow cartons.)

Eau-de-cologne is green and associated with old people. *S* was always given eau-de-cologne as a child when going to parties.

Peppermint is white (a natural association).

Iodine is dark bluish green (*S* cannot explain why.)

Carbolic acid is brilliant yellow.

Rose is black. (*S* thinks only very dark people should use this perfume.)

*Cutaneous Sensations*:

*a*) Temperature sensations:

Cold is white.
Heat is red.
Normal is yellow.

*b*) Touch has no color.

*c*) Pain has only one color sensation, namely, toothache, which is green.

*Kinaesthetic Sensations*:

No colors are associated with kinaesthetic sensations except in classical dancing, and then not the movement but rather the visual image of the poise is colored. For example, the Greek poise of terror is black; of despair, brown; of prayer, blue; and of soaring up, white. This last is kinaesthetic more than visual, and is accompanied by sensations from the glands and the muscles.

*Organic Sensations*:

Hunger is green. Thirst has no color, but is connected with the idea of water.

*Higher Senses*:

We have already considered in part the rôle of synaesthesia in the higher senses of our subject. The usual association in synaesthetic individuals appears to be between sound and color. The sound of a word produces that word as a visual image in space in front of *S*, and the visual image is colored. In the present case the association is much more elaborate. Any one word or even letter may give rise to *three* different sets of colors.

The colors vary according to whether *S* is thinking of

*a*) the look of the word or letter, that is, visual stimulus or image.

*b*) the sound of the word or letter, that is, auditory stimulus or image.

*c*) the idea of the word or letter.

*Example*: the consonant *B*.

Look of *B* is blue (because of liquid roundness of loops making the letter).

Sound of *B* is white (due to vowel *e*, which is white).

Idea of *B* is blue (is associated with name Betty and a childhood experience).

Sometimes only two colors are associated.

*Example*: word *Bus*.

Look of *Bus* gives rise to a visual image uncolored.

Sound of *Bus* is brown.
Idea of *Bus* is fawn.
*Example*: word *Bud.*
Look of *Bud* uncolored visual image.
Sound of *Bud* either real or imaged is brown.
Idea of *Bud* is pink (due to rose-bud).

In the case of vision, the word itself appears as an uncolored visual image: the associated color occurs within the individual, and permeates her whole body as a color atmosphere or a "color vapor." A colored sound is different, for the sound itself is colored, and may be outside *S*, in addition to creating an atmosphere within.

Most words take their characteristic color from the vowels they contain, for consonants as consonants have no color. Although *S* has a complete color alphabet, the colors of the consonants are for their names, and not for their sounds, e. g., *C* is blue, but *C* as a consonant has no color.

A series of words like *but, bus, bun, bud,* are accompanied by no color until sounded, and then are immediately brown, because the vowel *u* is brown, that is, the consonants have no influence in determining the color. The sound of the word in inner speech

TABLE 1

COLORS OF ALPHABET

| | | | | | |
|---|---|---|---|---|---|
| *A* | straw | *J* | straw | *S* | indefinite |
| *B* | blue | *K* | straw | *T* | white |
| *C* | blue | *L* | yellow | *U* | brown (light) |
| *D* | black | *M* | rose | *V* | violet |
| *E* | white | *N* | pink | *W* | water (*W*= bubbling brook) |
| *F* | pink | *O* | black | | |
| *G* | green | *P* | white | *X* | white |
| *H* | auburn | *Q* | indefinite | *Y* | grey |
| *I* | blue | *R* | indefinite | *Z* | queer yellow brown (striped) |

VOWELS

| | | | | | |
|---|---|---|---|---|---|
| *a* | (as in *hay*) | straw color | *i* | (*in*) | lavender |
| *a* | (*ah*) | deep red | *i* | (*height*) | bright blue |
| *a* | (*pan*) | brown | *o* | | black—the longer the *o* the blacker |
| *e* | (*keen*) | white | | | |
| *e* | (*den*) | pale straw or often color of water | *oo* | ("*oona*") | pale peat smoke blue |

| | |
|---|---|
| *au* | brown |
| *ae* | yellow |
| *ai* | deep yellow |
| *ou* | nut brown |

is almost simultaneous with its visual appearance, and it is only when *S* deliberately holds back the sound that this phenomenon appears. This came out very clearly in an experiment to be described presently.

Table 1 shows *S*'s color alphabet. These colors were written down by *S* two years ago, and a second communication given at the time of writing this article shows them to have remained unaltered. Only three are indefinite, *Q, R,* and *S*. It will be noticed that the vowels vary according to their sound. The *look* of *A* is scarlet, because of its sharp angles; the look of *a* has no color.

TABLE 2

| | |
|---|---|
| **Days of Week** | |
| Monday | Pale pearly grey (sometimes silver) |
| Tuesday | Prussian blue |
| Wednesday | Light yellow (associated with yellow pansy) |
| Thursday | Brownish orange (associated with bronze chrysanthemum) |
| Friday | Bright bottle green (a jade tinge) |
| Saturday | Pillar box red (Saturdays in past are crimson; Saturdays in future are navy blue) |
| Sunday | Pale but brilliant gold |
| **Months of Year** | |
| January | Pearly grey (cold) |
| February | Deeper tone of same (cold) |
| March | Sometimes brown (chocolate)—sometimes indefinite green (green when connected with thoughts of wind—although wind is not green) |
| April | Daffodil leaf green |
| May | Pink (not nice pink—rather magenta in tone, i. e., harsh) |
| June | Rose pink (coolness) |
| July | Warm foxy brown (heat) |
| August | Brownish sand color |
| September | Wheat color |
| October | Dark nut brown |
| November | Dusky grey |
| December | Grey with white and scarlet patches |

Table 2 shows the colors associated with the days of the week and months of the year. The colors of the days of the week are the most vivid of all *S*'s colors, and the same colors have been connected with them since childhood. *S* can give little explanation of them except for Thursday—"the 'urs' curls round like chrysanthemum petals." *Vendredi,* however, is jade green.

In the months of the year, the associated colors are more what

the idea of the month means for *S*. It will be noted that March has two colors associated with it, sometimes brown, sometimes indefinite green, when connected with thoughts of wind. The curious thing is that wind itself is not green but is blue-grey, although the sound it makes and the feel of it on *S* is white, because it is cool and exhilarating. December is grey with white and scarlet patches. These patches represent Christmas. New Year's Day is white. Hallowe'en is red. The 1st of August is brown. Birthdays are colored from the color of the number, e.g., 9 is blue, 8 is golden brown, etc.

In addition to having color associations with days of the week and months of the year, *S* visualizes them in space arranged in a particular shape or form. It would seem that all her thinking must be made concrete and translated into visual imagery. Figure 1 shows the form the days of the week assume. Note the two different standpoints. If the day is not a Friday or a Saturday, *S*

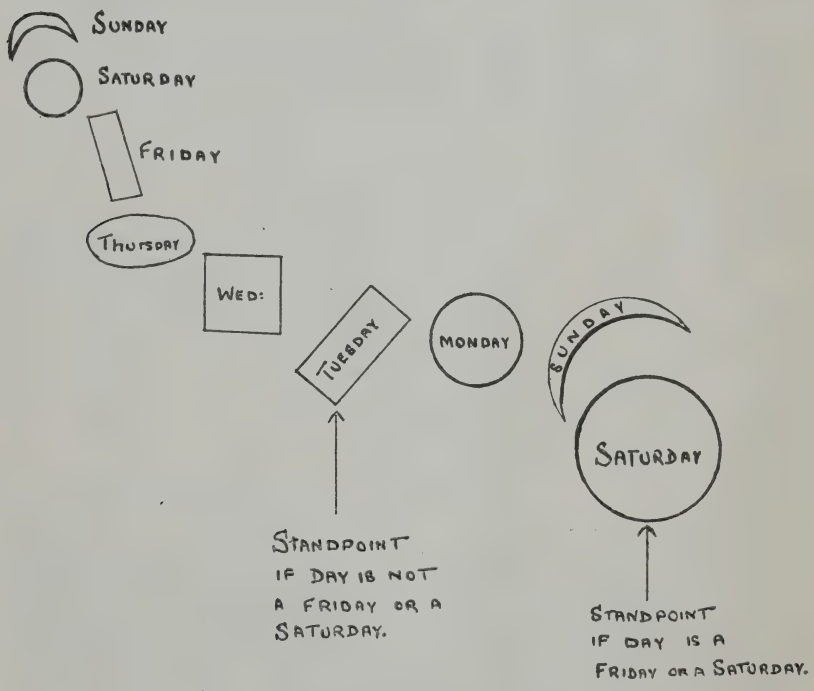


FIGURE 1
A WEEK

is at Tuesday, not definitely facing Tuesday but facing the whole week from this direction. In this case the first Saturday is omitted. If the day is a Friday or a Saturday the standpoint is at Saturday. This is the only occasion when Saturday is the biggest day and the largest circle.

The visualization of a number of weeks appears as a straight line of waves for a whole year, each wave representing a week. The figure is seen on a plane below and in front of *S* as if she herself were not living in the middle of time.

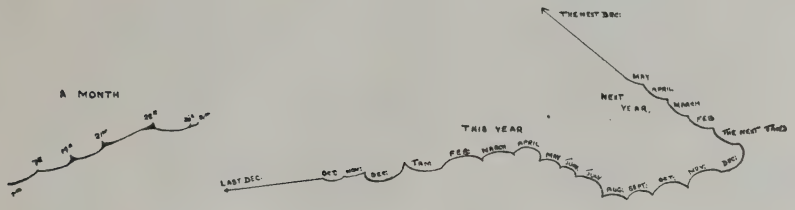


FIGURE 2
MONTHS OF YEAR

Figure 2 shows the form for the months of the year. This form shows the limits of *S*'s thinking in months. After May next year it is a straight line upwards to the next December, and "I can think no further in months, only years." After October of last year, it is a straight line going back in time to December of last year, and "I can think no further in months, only in years." This diagram on the months is seen on a plane below and in front of *S*, as in the form for the weeks. *S* cannot think in weeks and months together, except theoretically with no picture, for more than one month. The colors for the months are not present, but the winter is dull greyish, and the summer brighter, a haze of faint color. August stands out as the usual summer holiday month.

For months looking ahead and back the form is exactly the same as for the weeks, a series of equal waves.

A month is thought of as divided into week-ends, and the week-ends are always thought of as falling on the 7th, 14th, etc., although *S* realizes that this is not always the case. (*Vide* Figure 4.)

Figure 3 shows the form for days of the week looking ahead and looking back into time. In *looking ahead* the perspective is out into space, not upwards. The form is as much as *S* can grasp and realize at once. To think of days further ahead, *S* has to resort to other diagrams. *Looking back* in time the perspective is out into

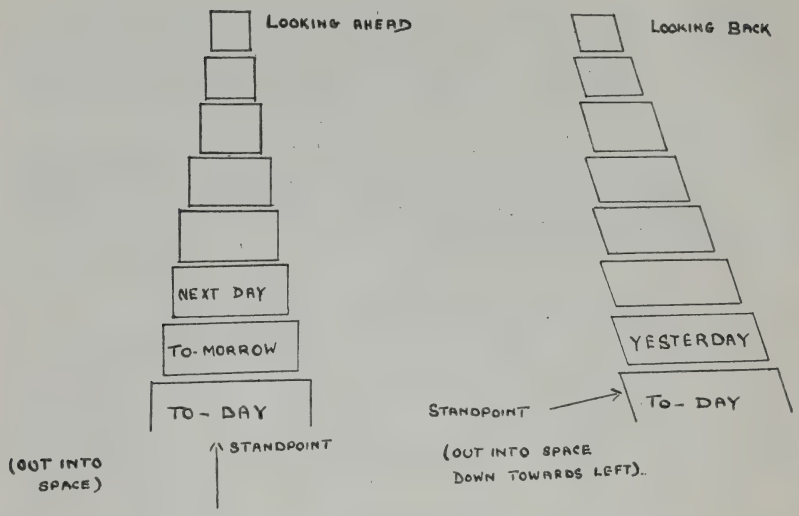


FIGURE 3
DAYS OF WEEK

space and downwards towards the left. Here again there is as much as *S* can realize.

Figure 4 shows *S*'s general form for time. From the present, the past goes at an angle of 45 degrees downhill over the left shoulder. In the future the year 2000 stands out and then the form goes uphill in front and to the right of *S* at an angle of 45 degrees. The last part of the diagram is general imagination of the past. The diagram is different when *S* is thinking of the past history of different nations.

Figure 5 shows one of the forms for past history, that connected with Jewish history. The upper form shows the childhood idea, the lower one the form in use now. *S* has different forms for different nationalities. The form for French history is a spiral whose center is at Paris with the French Revolution at the center. *S* is looking down on the spiral.

Figure 6 shows *S*'s number form. Two years ago this number form was changing, and both are shown in the figure, (1) and (2), the old and the new. Diagram 3 is common to both forms, and *S* finds no difficulty in the fact that 99 of (2) faces in a different direction from 100 of (3).

The fourth diagram is used in thinking financially. In adding and subtracting money, *S* thinks in twelves and twenties going upwards in a straight line on a vertical plane before her eyes.

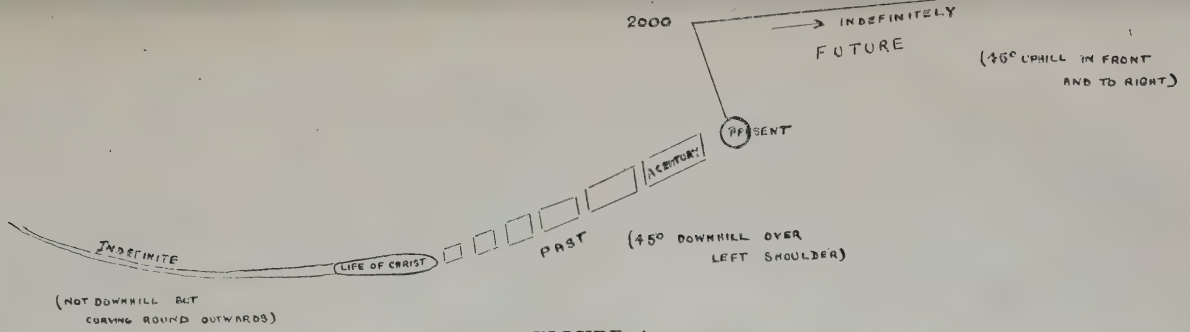


FIGURE 4
TIME

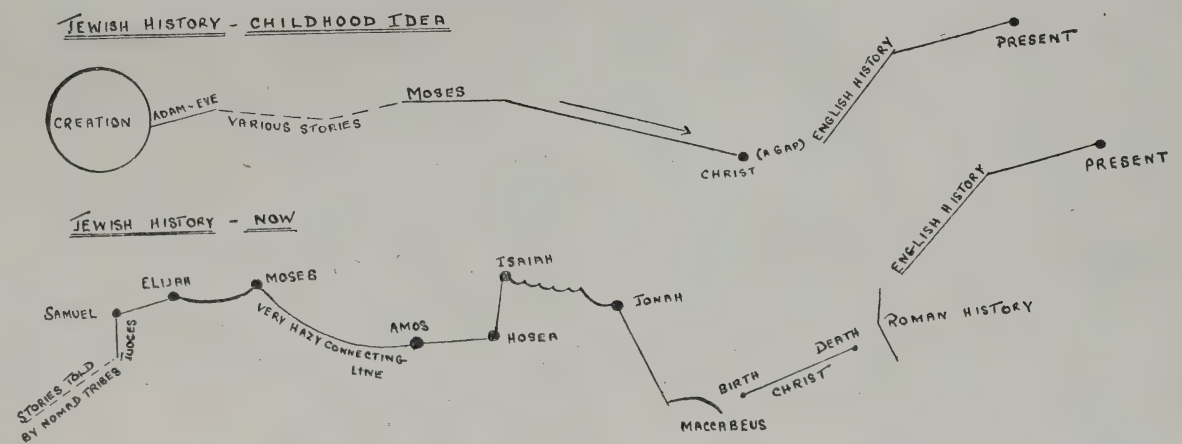


FIGURE 5
"TIME IN THE PAST" FORMS

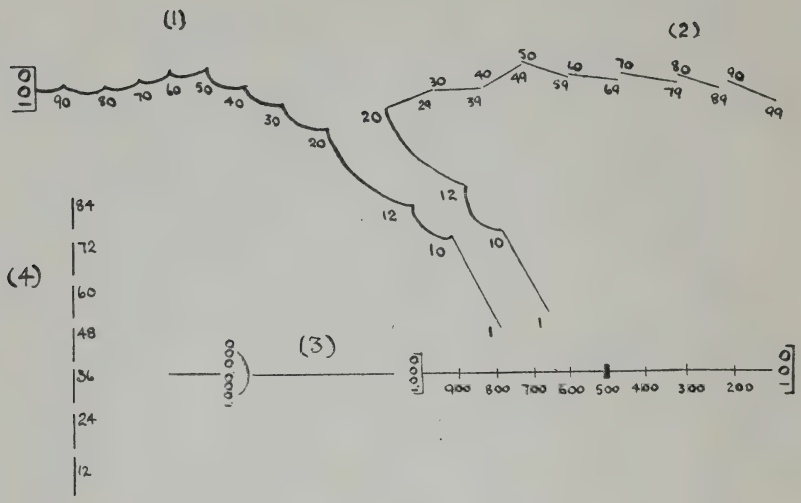


FIGURE 6
NUMBER FORMS

In all adding and subtracting, the number form is used. But in multiplying and dividing, *S* voluntarily banishes the forms from her mind. At the present time the number form has again changed, is much simpler in character, and includes negative numbers. The number 500 has now a conspicuous place because of 500 cc. in chemistry, a subject which *S* is at present studying.

The number form as a whole is uncolored, but any part of it in use is colored, that is, sections thought of separately are colored, though not very vividly.

Although the number form is not colored, each number has a color of its own, which appears when the number is thought of separately.

1 straw colored
2 blue of the sea
3 pale yellow
4 wallflower reddish brown
5 deep pansy yellow
6 white
7 bottle green (same idea as Friday)
8 brownish orange (same idea as Thursday, the two are connected in *S*'s mind)
9 Prussian blue (and Tuesday)

10 black. 10 is the same as the vowel *o*, a very vivid black. 10 is connected with ivy, and also makes *S* think of death and old-fashioned things. Her grandparents and aunts lived in a house, number 10.

11 not so dense a black
12-19 not colored (mere figures in space just outside *S*'s head)
20-29 not colored
30-39 yellowish color
40-49 reddish color
50-59 orange
60-69 white and water

70 onwards not colored, the only exception being 150 (same orange as 50, also 500).

*S* and her brother are both afraid of certain numbers, although not the same ones. *S* finds it very difficult not to consider 2, 4, and 13 unlucky, and 11 and 9 very lucky.

From the foregoing description of this case, it is undoubted that the synaesthesia or the psychochromaesthia, as some prefer to term it, is very extensive. Not only in the waking life of the individual does color play an important part, but in the dream life the colors are equally vivid. It is difficult to summarize or to draw conclusions from such varied yet regular data, but to get any understanding of the phenomena an attempt must be made.

1) It seems possible to conclude, although we have only one case under consideration, that synaesthetic individuals must possess good powers of imagery. It is undoubted that in this case the imagery in all sense departments is extraordinarily vivid. As already indicated, *S* possesses in addition eidetic imagery. In embroidery or in wood painting, *S* very rarely draws the design. She can picture the design so vividly that it is virtually on the cloth or on the wood. Any image can be projected on an external object. If it is a very intricate drawing, *S* can project it very clearly on a paper in front of her, so clearly that it would seem that all she has to do is to run the pencil along the lines of what she sees. This is not always satisfactory, for often on concentrating on the part where the pencil is, she momentarily loses the whole picture. Many of the forms, those for the days of the week, the months, time, etc., are nothing but examples of concrete visual imagery. Many people have this tendency incipiently.

2) Although the origin of many of the colors associated with words, objects, individuals, etc., cannot be traced, others are clearly due to associations. Saturday, at one time colored red, was for a time navy blue. This is the color of the Guide uniform, and the Guides to which *S* belonged met on Saturdays. Saturday is

now red again, because of the vowel *a* in Saturday. Fairies in childhood were colored pink and sky blue, but are now green and grey owing to reading people like Yeats and Fiona McLeod. *S*'s automobile, in reality fawn colored, is always thought of as red, because the first car of this make seen by *S* was red. The color of one individual is pink because when she was six years old he washed her hands with rose soap. Other examples could be multiplied. The association of object and color is in many cases a purely extraneous one, due to accidental circumstances.

3) Psychochromes change in some cases. This is due to added experiences. Others, such as the colored alphabet, remain unaltered.

4) Many color associations follow regular rules. The associated colors are due mainly to the influence of the vowel sound. Saturday is red, because of the *a*; dog is black because of the *o*.

5) To a certain extent, the results from this *S* agree with those reported by Wheeler and Cutsforth in recent numbers of the *American Journal of Psychology* and the *Journal of Experimental Psychology* as regards the place of color in the mental life of synaesthetic individuals. Their main conclusions are that the associated color does not play a secondary rôle, but is an essential part of the mental equipment of the individual. That is, the so-called secondary process is essential rather then incidental in the mental life of the synaesthetic individual.

*S* writes in support of this: "Days of the week, people that I know, many names, all scents, and all tastes that are colored never change their color, and I cannot think of these apart from their color," and again, "I use colors very largely in remembering facts, and I group facts and names, for example, of parts of the body which are 'like' each other, the likeness consisting in a hazy color background."

These statements were clearly confirmed in the results obtained from two experiments after Wheeler and Cutsforth.

In the first experiment *S* sat with closed eyes, and letters cut out of glass paper and mounted were placed successively before her. *S* had to identify the letters by touch and introspect. No color appeared until the letter was recognized and whenever the letter was identified, the color simultaneously was evoked. The tactual stimulus and recognition of the letter called forth a visual image in space followed after an almost imperceptible interval by the name of the letter. For example, when *A* was the letter, *S* while tracing it exclaimed, "That's red,"—the shape was sufficient in this case. When the name of the letter came into consciousness, a straw color was its accompaniment.

With some letters there was no color until the letter was identified

by name. This was brought to light by *S*'s deliberately trying to repress the name.

The second experiment was to learn a series of 10 nonsense syllables which were presented to *S* successively with a regular interval between each two. The strip of paper containing the nonsense syllables was fastened on a rotating cylinder whose rate was constant. The learning and saving method was used.

After the first presentation of the series, *S* could remember that some of the syllables were black, some red, some white, and some grey, the syllables with *o* being black, with *a* red, with *e* white, with *i* grey. Two black ones were remembered, two red ones, and one grey one. Only much later did *S* notice one with *u* as brown. The syllables were grouped in this way in succeeding repetitions. When *S* was reminded that she would have to produce them in the order given, she had to break up this grouping and try to hold syllables back to fit them into their proper position. All the syllables were visualized as on a straight vertical line, with colors standing out; *o* as black was the most vivid, red was also vivid, white and grey were not so vivid, and brown was not vivid at all. An unusual case was the word *loy,* which appeared black with purple in it, and which remained unobserved by *S* until near the end of the learning.

One interesting feature showing the strong visualizing powers of *S* was that, at the fifth presentation, *S* started at the foot of the list and worked up. It was as easy to do as starting at the top.

*S* declared that she could not have learned the series without colors. If the vowels had been left out, she perhaps could have laboriously learned them by mechanical repetition and sound.

A further experiment in learning was carried out by giving *S* nonsense syllables as before, but having the syllables painted this time in colors different from those with which *S* would associate them. The result was interesting. The colors actually presented were ignored, and *S* saw them before her quite distinctly in their own colors. It was simple for her to suppress the real colors and substitute her own.

Color does seem to fill an essential place in *S*'s mind, and seems to act, as Wheeler and Cutsforth express it, in a manner analogous to that of a conditioned reflex.

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## UN CAS DE SYNAESTHÉSIE

(Résumé)

1) La synaesthésie est très développée et comprend des personnes, des conférences, des livres, des auteurs, de la musique, des pays, et tous les domaines des sens, sauf celui de la kinaesthésie.

2) Le sujet possède des formes de nombres; des formes pour les jours de la semaine, jours passés et jours prochains; des formes pour les mois, passés et prochains; et des formes pour l'histoire.

3) Le sujet a toujours conscience d'une atmosphère présente de couleurs, laquelle change selon la personne adressée, la matière écoutée, la musique écoutée, etc.

4) N'importe quel mot ou lettre peut causer trois groupes différents de couleurs. Les couleurs peuvent varier selon que le sujet pense à l'apparence, au son, ou à l'idée du mot ou lettre.

5) Le mot lui-même se présente comme une image visuelle non colorée.

6) La plupart des mots ont leur couleur caractéristique selon les voyelles qu'ils contiennent.

7) Quelques-unes des couleurs sont dues à des associations.

8) La couleur associée ne semble pas jouer un rôle secondaire, mais joue un rôle essentiel dans la vie mentale de l'individu.

*a*) Dans une expérience d'apprentissage où l'on a présenté les lettres tactilement la couleur s'est présentée au moment où chaque lettre a été identifiée.

*b*) Dans la seconde expérience, l'apprentissage de dix syllabes non-sens, on a trouvé que les syllabes ont été groupées selon leurs couleurs.

COLLINS

## EIN FALL VON SYNAESTHESIE

(Referat)

1) Die Synaesthesie ist hoch entwickelt und umfasst Menschen, Vorlesungen, Bücher, Autoren, Musik, Länder, und alle Sinnesgebiete, ausser Kinaesthesie.

2) Die Vp hat Zahlformen; Formen für die Wochentage, für die kommenden wie für die vergangenen; für Monate, ebenfalls für die kommenden wie für die vergangenen; und für Geshichte.

3) Die Vp ist sich immer einer Farbenatmosphäre bewusst, die mit jeder angesprochnen Person wechselt, wie auch mit dem Gegenstand des Gesprächs, oder mit der Musik, die sie hört, etc.

4) Irgend ein Wort oder Buchstabe kann drei verschiedene Farbenkombinationen hervorrufen; diese können varriieren, je nach dem, dass die Vp an das Aussehen, den Klang oder die Idee des Wortes oder des Buchstabens denkt.

5) Das Wort selber erscheint als farbloses, aber sichtbares Bild.

6) Die meisten Wörter erhalten ihren Hauptfarbenton von den Vokalen, die sie enhtalten.

7) Manche Farben werden durch Ideenverbindungen herbeigeführt.

8) Die Assoziationsfarbe spielt, scheinbar, keine untergeordnete, sondern eine sehr wichtige Rolle im Geistesleben des Individuums.

*a*) Bei einem Lernversuch, wobei die Buchstaben durch Tasten vermittelt wurden, nahm die Vp die Farbe wahr, sobald sie den Buchstaben erkannte.

*b*) Bei dem zweiten Versuch, dem Erlernen von 10 Unsinnsylben, fand es sich dass die Sylben nach ihren Farben geordnet waren.

COLLINS

## СЛУЧАЙ СИНЭСТЕЗИИ

(Реферат).

1. Синэстезия очень распространена и может быть прослежена на различных людях, при чтении книг, творчестве, в музыке; она может быть прослежена в условиях отдельных стран, на материале различных рецепторов за исключением осязательного.

2. Наш суб'ект характеризуется образным восприятием чисел; он имеет образы для дней недели и месяцев (безразлично—прошедших или будущих) и для исторических событий.

3. Суб'ект постоянно имеет цветовую характеристику состояния атмосферы, которая меняется в зависимости от того, с кем он говорит, воспринимаемого материала или слышимой музыки.

4. Каждое слово или буква вызывали различные цветовые ощущения в зависимости от того, на какую сторону: оптическую, звуковую или смысловую суб'ект обращал внимание.

5. Слово само по себе воспринималось, как неокрашенная зрительная форма.

6. Большинство слов принимали характерную окраску в зависимости от содержащихся в них гласных.

7. В некоторых случаях цветовая окраска зависела от ассоциаций.

8. Ассоциированная окраска играет значительную роль в психической жизни данного индивида.

а) В пробных опытах, где буквы пред'являлись тактильно, цветовая окраска появлялась лишь при идентификации буквы.

в) в другой серии — было установлено, что при заучивании бессмысленных слогов, они группировались по цветовому признаку.

**Коллинс (Collins).**

# THE PSYCHOPHYSIOLOGY OF AUDITORY QUALITIES AND ATTRIBUTES*

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## I. INTRODUCTION

The highly suggestive paper by my colleague, Professor Boring,[1] dealing with auditory theory, appears to have had the desired result, in stimulating renewed discussion of this intriguing topic. Criticisms by Banister,[2] Wilkinson,[3] and Gundlach[4] have raised a number of interesting issues, and a further examination of the facts in relation to the theoretical possibilities seems not to be out of order. A survey of recent papers, dealing with these problems, reveals an unfortunate lack of appreciation, on the part of certain writers, of the points which have mainly influenced the thoughts of others. Thus, the authors above mentioned neglect what seems to me to be the paramount significance of the recent studies in audition made by members of the Bell Telephone Laboratories staff, under the leadership of Dr. Harvey Fletcher.[5] On the other hand, the physical bias of this latter group of investigators has prevented them from giving due recognition to the neurological factors which are inevitably involved. Boring bases his arguments to a considerable degree upon the psychophysics of auditory volume, while Banister doubts the sensory reality of this characteristic. Boring, on the other hand, leaves auditory "brightness" quite out of his account, and none of the writers above mentioned have laid sufficient stress upon the psychophysiology of noises and vocables, as contrasted with musical tones.

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[1]Boring, E. G. Auditory theory with special reference to intensity, volume, and localization. *Amer. J. Psychol.*, 1926, **37**, 157-188.

[2]Banister, H. Auditory theory: a criticism of Professor Boring's hypotheses. *Amer. J. Psychol.*, 1927, **38**, 436-440.

[3]Wilkinson, G. Is the question of analysis of sound by resonance in the cochlea or by "central analysis" in the brain still an open one? *Amer. J. Psychol.*, 1927, **38**, 257-265.

[4]Gundlach, R. H. A note on auditory theories. *J. Exper. Psychol.*, 1927, **10**, 64-66.

[5]See: Fletcher, H. Physical measurements of audition and their bearing on the theory of hearing. *J. Franklin Instit.*, 1923, **196**, 289-326.

It is far from being my purpose to pose as an oracle in this field of psychophysiology, but I have recently been under the necessity of examining the facts and hypotheses from a general standpoint, and have noticed certain theoretical possibilities which do not seem to have duly affected contemporary discussions. In particular, I feel impelled to defend certain of Boring's persuasions, although in association with assumptions which differ considerably from his own. Furthermore, it has seemed to me that a contribution could be made to current thought concerning this topic among psychologists by emphasizing some facts and hypotheses which seem not to have received due recognition in their minds. I am astonished to find that practically none of the psychological writers seems to be familiar with the principle of "non-linear distortion" or "rectification," as applied to the auditory sense-organ. This principle provides a basis for the immediate explanation of a number of salient auditory phenomena, such as difference and summation tones, subjective harmonics, and dissonance; and can be extended to account for other relationships, such as those involved in "octave quality," the "sense of interval," etc. I have been unable to find any evidence that writers of psychological textbooks are acquainted with the existence of subjective harmonics, although these are everyday phenomena to the workers in the Bell Telephone Laboratories. The principle of non-linear distortion is, of course, at the very heart of modern radio practice, and presents one of the outstanding difficulties in the design of satisfactory audio-amplifiers and electro-acoustic devices.

The physical workers, on the other hand, seem content to neglect the properties of nervous tissue—the all-or-none law, refractory phase, and the like—or to give their implications only secondary attention. They also fail entirely to consider certain features of auditory experience which are of great interest to the psychologist. These include volume, brightness, consonance, fusion, octave quality, and certain aspects of localization. Now, it seems to me that recent advances in the physiology of the nervous system and its tissues have provided us with principles, on the basis of which we may hope to arrive at a new insight into some of the classical psychophysiological problems. In this domain we are apparently entering a period of *quantum* theorizing. We have always been compelled to deal with spatial quanta, in the form of individual conducting nerve fibers, but the all-or-none law has enforced a similar requirement in the temporal dimension, and has made the situation correspondingly more clear-cut. Certain neuro-psychological explanations which seemed simple and obvious to the older thinkers, such as Helmholtz, are now definitely debarred. The intensity of the stimulus can no longer be represented by the supposedly variable amplitude of the nerve impulse.

The difficulty of the theoretical problems seems to be increased, but, at the same time, the accompanying reduction in the number of possibilities improves our chances of finding the correct answer. The recent papers of Hecht,[6] dealing with the relation of visual acuity and brightness, introduce a statistical treatment of nerve quanta, which is reminiscent of similar methods in physics, and should prove to have many other important applications.

I trust that the present paper may make some slight contribution to the unification of pertinent principles in the domain of auditory theory. Following Boring's example, I wish to disclaim any purpose of establishing a supposedly new hypothesis, as I shall advocate certain views, not because I feel confident that they are true, but because they merit more defense than they have thus far received. In this way, we may advance the cause of auditory theorizing rather than that of some particular theory, as Boring has desired.

## II. The Facts Which Require Explanation

What passes as "auditory theory" appears to be an attempt to establish psychophysical relations between the characteristics of auditory experience and the processes of the afferent arc. These latter include the mechanism of the sense-organ, the receptor excitation, and the features of the sensory nerve currents. Common observation, plus laboratory experiments, usually establish functional relationships between the experiences and certain aspects of the sonorous body or the resulting sound waves. Thus, we find that pitch is a semi-logarithmic function of frequency, that localization varies with phase difference at the two ears, and the like. As a rule, these psycho-stimulus functions are rather complex, and it would be pleasing if we could show that the dependencies of the experiential facts upon the afferent nerve currents or upon the receptor processes are somewhat simpler. At any rate, we are interested in ascertaining the exact nature of these more intimate relationships.

The most straightforward method of arriving at such knowledge would seem to be to determine, firstly, the functional relations which connect the characteristics of the stimulus with those of various segments of the afferent arc: the sense-organ response, the receptor and the afferent nerve excitations. This would naturally be done in a series of steps, according to the order in which these various phases succeed one another in space or in time. Such a determination could conceivably be made by purely physical or physiological methods. Having ascertained these stimulus-receptor functions, or the like, they could be combined logically with the known psycho-stimulus functions and the stimulus factors could be cancelled out, leaving the desired answers. If it should transpire that such answers

[6] Hecht, S. A quantitative basis for the relation between visual acuity and illumination. *Proc. Nat. Acad. Sci.*, 1927, **13**, 569-574. Also, *J. Gen. Physiol.*, 1928, **11**, 255-281. For a more general discussion of nerve quantum principles see: Adrian, E. D. The basis of sensation. New York: Norton, 1928. Pp. 122.

present a simplified set of psychophysical relationships, we might then say that we had achieved a certain degree of "explanation," many of the complexities having been shown to be resident in the stimulus-receptor relations, only. For example, it appears that, in certain cases, the familiar logarithmic law belongs in the latter class and is not a true psychophysical principle, as Fechner believed it to be.

Now, a recital of all of the relational facts which need to be dealt with in this manner would involve writing a textbook chapter on the subject in question. However, they may be outlined briefly as follows. Firstly, we must account for the fundamental distinction between tones, as accompaniments of distinct sinusoidal components in the stimulus, and non-tones (including noises and vocables) in their association with aperiodic or compositionally intricate sounds. Considering the tones, we must deal with the manner in which loudness varies as a function of frequency: the audibility curve and its congeners; as well as with its manner of determination by stimulus amplitude. The connection between loudness and the frequency composition of the sound, the masking effect, requires particular attention. Next, we must consider the dependency of pitch upon frequency, and its seeming independence of amplitude. In relation to composition, the all-important facts of Ohm's law must be treated in an adequate manner. We must understand not only why analysis sometimes occurs, but also why it is frequently quite impossible. The determination of "brightness" by frequency and, independently, by the distribution of energy in the acoustic spectrum, as in vowel sounds, must also be accounted for; and we must not forget the variations of volume with frequency, intensity, and binaural phase difference.

In discussing non-tonal auditory qualities, we must deal with the qualitative varieties of noises, vowels, consonants, etc., and their established correlations with definite distributions of acoustic energy with respect to frequency. We must consider their similarities to tones, especially in regard to loudness, pitch, and brightness.

We shall be compelled to treat, furthermore, of the complications which arise when a comparatively simple stimulus is operative so as to yield a relatively high loudness. If the stimulus is of a single frequency, there may be subjective harmonics; or if it has two or more sinusoidal constituents, there will ordinarily be difference tones of various orders, and possibly a summation tone. Then, there are the complex phenomena which attend beats: the intermediate tone and its changes, the variations in the quality of the beats with frequency and frequency difference, the question of the relation of beats to difference tones.

Passing to less sensory facts, we may hope to throw some light

upon the conditions and nature of tonal fusion, the foundations of consonance and dissonance, of octave quality, tonality, and the like.

Our views must also be consistent with numerous details of the temporal course of auditory experience with respect to that of the stimulus, such as its relatively low inertia and adaptability, its independence of phase except when the latter is suddenly altered, etc. Perhaps the most important point of all will lie in a consistent treatment of the facts of binaural localization on the basis of phase differences or of differences in the timing of the stimuli at the two ears.

## III. The Mechanism of the Auditory Sense-Organ

I shall neglect to enumerate in detail the long list of hypotheses which have been advanced concerning the mechanism of the auditory sense-organ, since they have been adequately summarized by other writers. Many of them appear to have been the products of armchair speculation, rather than of an analytical study of the structure of the ear and its probable behavior in accordance with established dynamical principles. It should be possible to ascertain the nature of the response of this organ by the application of positive mathematical methods, given its mechanical constants and the specifications of the stimulus. Although the required data are not available in accurate form, we can nevertheless establish a qualitative line of reasoning which is highly fruitful, and which pushes the realm of vague guesswork along the auditory pathway a considerable distance. I am referring to the notion of the mechanics of the middle and inner ear which has been developed by Wegel and Lane,[7] and their collaborators of the Bell Telephone Laboratories. Their views seem to me to be highly convincing in many important respects. Owing to the apparent lack of conversancy of psychological writers with these developments, it seems worth while to outline them in some detail.

It will be recalled that the acoustic vibrations which impinge upon the tympanum are transmitted through the ear-bones, to the membrane which covers the "oval window." At this point, they are applied to the endolymph of the scala vestibuli. Now, if the frequency of the acoustic vibrations is very low—say, less than twenty cycles per second—the motion is very effectively transmitted, but the changes in pressure are so slow that there is plenty of time for the liquid to move back and forth through the

[7] Wegel, R. L., & Lane, C. E. The auditory masking of one pure tone by another and its probable relation to the dynamics of the inner ear. *Phys. Rev.*, 1924, **23**, 266-285.

helicotrema, the opening which connects the scala vestibuli with the scala tympani at the apex of the cochlea. The pressures upon the basilar membrane are thus practically the same on both faces of the latter, and it undergoes no appreciable displacement. The tension or stiffness of the membrane is of course involved in determining this action, and the pressure changes must find an outlet at the membrane of the fenestra rotunda. This is supposed to be the condition at the lower frequency limit of audibility.

As the frequency is increased, the mass of the fluid will count more and more, and the stiffness of the basilar membrane less and less so that the latter will be subjected to an increasing oscillatory displacement. Mechanical analysis, as well as experience with analogous electrical filter networks, shows that this displacement should exhibit a maximum near the helicotrema for very low frequencies, but that the position of the maximum should progress towards the base of the cochlea, as the frequency advances. At high frequencies, there will be substantially no longitudinal motion of the endolymph, the entire disturbance being transferred across the membrane, very close to the oval and round windows. Thus, although the basilar membrane will tend to move in its entirety for any frequency in the audible range, it will show a maximal amplitude at a point along its length which is definitely localized as a function of the frequency in question. At excessively high frequencies, say above 30,000, the total mass of the transmission system, including the ossicles, will be so great that practically no vibration is imparted to the inner ear media and tissues. This represents the condition for the upper limit of audibility.

Now, the net result of this analysis is evidently very similar to that of the Helmholtz hypothesis. Moreover, the mechanism is one of resonance. But the method of reasoning differs from that of Helmholtz in important respects. In the first place, the basilar membrane is not supposed to consist of a series of individualizable resonant units; and, secondly, although the *stiffness* of the membrane is an important factor in the action, the *mass* which is involved is primarily that of the endolymph. The existence of the helicotrema is a very important item in the working of the system. The argument is based upon the integral behavior of all of the mechanical factors in the auditory apparatus, and not upon a conception of the basilary structures alone. It is best understood in terms of an analogy with a corresponding electrical network, in which the mass of the endolymph is replaced by a distributed inductance, the stiffness of the membrane by capacities between various points in the inductive circuit, the viscosity factors by appropriate resistances, etc. An alternating current of fixed voltage, but variable frequency, would

produce currents through the capacities, changing in the location of their maxima as a function of the frequency, in such a system. It is unlikely that such a view of the auditory mechanism could be developed outside of a telephone research laboratory, and it is relatively difficult for those who are not alternating current engineers to appreciate its full significance.

In harmony with this theory, there can be no question as to how many basilary fibers or *"strings"* are responding to a given stimulus. The entire length of the membrane responds to some degree to all frequencies; the location of the point of maximal amplitude is supposed to be the spatial criterion of frequency or pitch. Here again the result is the same as would be demanded by the Helmholtz theory, but the reasoning is divested of confusing artificialities. If the individual basilary fibers were actually tuned elements, the high degree of damping to which they must be subject would prevent them from exhibiting any very great sharpness of resonance. However, a sharply defined spatial delimitation of the response may actually exist when we come to the receptor cells and their attached nerve fibers. If the amplitude of vibration of the membrane at any point does not reach that which is required to pass the excitation thresholds of the corresponding receptors, the nerve current will be zero. But, if the threshold is passed, there should be maximal nerve response, in accordance with the all-or-none law. Thus, at very low intensities, perhaps only a single nerve fiber will be excited, and the identity of this will be definitely determined by the frequency of the stimulus.

In the case of a stimulus which involves more than one pure sinusoidal vibration, we should expect to find a pattern of oscillatory maxima upon the membrane, such as would be obtained by a superimposition of the amplitudes which are characteristic of the individual components, by themselves. If the intensity is low, there would presumably be the same number of maxima as there are sinusoidal components in the stimulus. However, if the intensity is high, this condition cannot be guaranteed, for the reason that a mechanism such as that of the ear almost inevitably introduces the effects of so-called non-linear distortion. This distortion is attributable to the fact that the displacement of the transmission mechanism does not follow Hooke's law, with mathematical exactitude, or is not at all times proportional to the acoustic pressure upon the tympanum. It is probable that the system of ear-bones operates somewhat after the fashion of an overloaded vacuum tube amplifier, or the "detector" of a radio set, emphasizing the inward movements as compared with the outward ones, or vice versa.

It can be shown mathematically, as well as by experiments with

vacuum tubes, that this form of distortion will introduce the following types of adventitious vibration: (1) a series of harmonics of the given frequency, which will increase in amplitude relatively to that of the fundamental, as the intensity increases, (2) when two or more frequencies are applied, a complex assembly of difference frequencies, and (3) under the same conditions, summation frequencies of various orders. The necessity of these conclusions is beyond dispute from the physical standpoint. Consequently, at relatively high levels of excitation, we should expect the pattern of vibration of the basilar membrane to be much more complex than that which accompanies very low intensities for a given frequency composition of the stimulus. Thus, with stimulus frequencies of 700 and 1200, Wegel and Lane found evidence of membrane maxima corresponding to the following additional frequencies: 1900, 500, 2400, 1400, 3600, 2100, 3100, 1700, 2600, 200(?), 2800, 3800, 1000, 4300, 2900, 3300, and 900. Each of these could be identified definitely by means of beats produced in relation to a third, "exploring," tone. With the exception of the frequency, 2800, the values found are what would be expected if the response, $x$, of the middle ear mechanism follows a non-linear law, according to the equation:

$$x = a_0 + a_1 p + a_2 p^2 + a_3 p^3 + a_4 p^4.$$

It is evident that these considerations satisfactorily account for the phenomena of subjective overtones, difference tones, and summation tones.

They also assist in explaining the fact that the masking of a note of higher frequency by one of lower frequency is usually much greater than the converse influence. In general, masking is attributable to the fact that any pure sinusoidal stimulus excites receptors at many points along the membrane, and not merely at the locus of its own characteristic maximum. The extent of this spread of the excitation naturally increases with an augmentation of the general membrane amplitude, but more in the direction of higher than in that of lower frequencies because of the appearance of maxima corresponding to the harmonics and combination tones. In order that a secondary frequency should become audible, it must bring about an appreciable change in the excitation in its own characteristic locality, and its threshold will naturally be increased in consequence of the already existing disturbance. The conditions for this threshold effect can be conceived in several different ways. According to Wegel and Lane, it is a matter of a just noticeable increase in amplitude, as such. However, it is more in harmony with the modern view of nerve response to suppose that the secondary frequency must excite hair-cells which are not already being excited

by the primary frequency. The asymmetry of the masking effect, on the frequency scale, is, of course, attributable to the distortion harmonics, which involve bands of the membrane above, but never below, that for the primary frequency. That masking is due solely to a feature of the sense-organ process is shown by its absence when the primary frequency is applied exclusively to one ear and the secondary frequency exclusively to the other ear. In practice, there is a masking action of about one per cent in such an experiment, which corresponds quantitatively to the value of "bone conduction" from one side of the head to the other.

Thus, up to this point, the argument provides an "explanation" of the following relationships between the stimulus and auditory experience: (1) the audibility curve, (2) tonal analysis, (3) subjective harmonics, difference and summation tones, and (4) masking. The bases of pitch and loudness are clearly suggested, but require a more detailed study, as does the closely related problem of analysis.

## IV. The Psycho-Neurology of Pitch and Loudness

Just as the neglected phenomenon of auditory masking seems to provide the means for a quantitative study of the basilar vibrations, so that of auditory localization by means of phase difference between the two ears has, for a long time, seemed to me to furnish the best beginning for a theory of the auditory nerve currents. This principle seems to demand, inevitably, a temporal representation of the actual stimulus frequencies in the auditory impulses. I have taught this in my classes for the last ten years, and I am therefore heartily in sympathy with Boring's method of attack upon the problem, and also with the "hypothesis" of Banister[8] in this matter. I am inclined to feel, however, that we have here to deal not with a hypothesis, but rather with an obvious necessity. If the assumption in question is inconsistent with current beliefs regarding the nature of the nerve process, then these beliefs must be in some way erroneous, as applied to the auditory fibers.

However, when I consider the details of Banister's auditory theorizing—particularly in his criticism of Boring—I encounter serious difficulties. Boring's discussion also seems to me to get into trouble when considered in the light of the researches of Wegel and Lane. These difficulties refer, firstly, to the details of the receptor process; secondly, to the manner in which pitch is represented in the auditory nerve currents; and, thirdly, to the way in which it is recognized by the brain. The majority of auditory theorists seem to feel that

[8]Banister, H. A suggestion towards a new hypothesis regarding the localization of sound. *Brit. J. Psychol.*, 1926, **17**, 142-153.

the fact of resonance in the sense-organ eliminates the need for any representation of pitch apart from the identity of the excited nerve fibers. On the other hand, defenders of the telephone or frequency theories are content to assign the problem of analysis to the brain without suggesting its probable mechanism at this point. Now, the reasoning of Wegel and Lane makes it almost certain that there is an actual analysis, at least of relatively simple tonal combinations, upon the basilar membrane, so that different frequencies are assigned to separate groups of auditory nerve fibers. Hence, we can at once relieve the cortex from this duty. It will probably have a sufficiently difficult task to perform in *keeping* the disturbances separated, even when they are delivered along distinct paths. On the other hand, there are difficulties in the view that pitch quality can be determined wholly on the basis of mere fiber identity.

But let us first consider Banister's hypothesis. He supposes that the impulses in the hair-cells are aroused at a fixed phase of the cycle of vibration of the basilar membrane, which constitutes the "effective position" of the latter. There is thus a definite temporal relationship between the stimulus vibrations and the pulses which are sent along the nerve fibers, and the difference in the timing of corresponding impulses from the two ears should be the same, at the brain centers, as the difference in timing between corresponding phases of the sound on either side of the head. This evidently provides a satisfactory basis for auditory localization in terms of phase difference. Now, there seems to be no reason for supposing that there is more than one nerve pulse for each basilar cycle. It therefore appears that, up to the limit which may be imposed by the refractory periods of the fibers, the actual audio-frequency which is impressed upon the tympanum is reproduced by the pulses of the auditory nerve currents.

Hence, it is somewhat surprising to find Banister objecting to a frequency theory of pitch, as advocated by Boring. He presents us with the essentials of a frequency theory, at least for tones up to about 500 cycles, and then denies that these factors are effective. However, if we combine Banister's teachings with those of Wegel and Lane, it appears that we are provided with a dual representation of pitch in the nerve process: it has a characteristic frequency and also a characteristic place. Which of these shall we regard as being the real determining factor?

Now, there are a number of considerations which may guide us in making our choice. In the first place, we may ask: If the audio-frequency in the nerve current, as demanded by Banister's reasoning, does not determine pitch, what feature of auditory experience *is* it responsible for? If we follow the principles which seem to apply

in other neurological domains, we may consider it to be determinative of loudness or subjective intensity. Such a supposition has interesting implications. It is generally considered that at the absolute threshold, tones are of equal loudness regardless of their pitches,[9] but if we consider the condition for the threshold to be the rhythmic excitation of a single basilar receptor, there must be a thousand times as many nerve pulses per second at 20,000 cycles as at 20 cycles. Hence, it seems necessary to suppose that more than one fiber may be involved at the threshold, and that, at 20 cycles, as many as a thousand may be required, about one-fourteenth of the total number in the auditory gamut. This is in harmony with the usual view (adopted by Hecht for vision) that intensity depends upon the total number of pulses per unit time along all cooperating nerve paths.

It is rather difficult to avoid these assumptions, and if we accept them, our picture of the nerve process for various pure sinusoidal stimuli of relatively low intensity must be somewhat as follows. The threshold for the highest frequencies may involve only a single fiber which responds in synchronism with the cycles of the stimulus, although possibly skipping some of the individual waves, if the refractory period of the fiber interferes. As the frequency is reduced, the number of fibers required for the threshold must increase, at any rate beginning at the point at which there is no "skipping," and below this frequency point must be inversely proportional to the frequency in question. If the highest non-skipping frequency is 500, then at 20 cycles at least 25 fibers must be involved at the threshold. All of these fibres must be supposed to be excited at the actual frequency of the stimulus, in synchronism. When the intensity of the stimulus is increased above the threshold, there must be an increase in the number of coexcited fibers, since there is no other means for representing the increase in the nerve current, and since the mechanism of the sense-organ calls for an effect of this sort only. Each just noticeable increase in loudness must require at least one additional fiber, and probably more than one. At 1000 cycles, 270 tones differing in loudness by a threshold amount can be discriminated, so that for the loudest of these at least 270 fibers will be synchronously innervated. These are minimal values, and the tentative findings of Wegel and Lane indicate a much greater spread of the excitation at high intensities.

This leads us to the consideration of recognized difficulty with all "place" theories; namely, that a single frequency must excite the places which belong, characteristically, to higher and lower frequencies. A simple and thoroughgoing place theory of pitch would

[9]Cf. Kingsbury, B. A. A direct comparison of the loudness of pure tones. *Phys. Rev.*, 1927, **29**, 588-600.

lead us, under these conditions, to expect an experience involving a mixture of adjacent pitches. Such experiences are psychologically possible, and are realized when the corresponding vibration frequencies are present in the stimulus or on the basilar membrane, but they do not occur in connection with the spreading of the excitation due to a single frequency. This fact constitutes a very serious difficulty for place theories, and one which becomes greater in proportion to the demonstrable degree of the spreading. If we study the mechanism of the cochlea, as presented by Wegel and Lane, it becomes highly probable that very low frequencies have a very extensive spread, perhaps over the entire membrane, since the endolymph must oscillate along the entire length of the scala vestibuli, and the resonance peaks must be very blunt. This is probably part of the reason why low frequencies have a greater masking effect than do higher ones. Even with a 1200-cycle note, the masking action is appreciable at the 600- and at the 3000-cycle positions, a probable span of more than one-third the entire length of the basilar membrane.

In order to avoid these difficulties, it is necessary to complicate the assumptions underlying a place theory, and to suppose that the cortex possesses a mechanism for basing its pitch reactions upon some particular member of the total group of excited fibers. Wegel and Lane assume that the position of maximal amplitude upon the basilar membrane determines this choice, but they do not explain how this idea can be made consistent with the all-or-none law. It would appear that, in accordance with Banister's hypothesis, the nerve currents along all of the coexcited fibers must be identical; both the amplitude and the frequency must be invariable. Even a difference in phase seems to be excluded. However, there are still some possibilities, some of which have been considered by Hartridge.[10] It is unlikely that all adjacent receptors have the same thresholds, so that excitation of every receptor in a continuous band may require a considerable amplitude of the membrane, lesser amplitudes bringing about a scattering of the excitations. But this line of thought necessitates even greater spatial spreading of the excited zone than as if the thresholds were all alike. Another possibility is that the cortex works from the edges of the excited band of fibers, and bases pitch upon a computation of the mid-position. In this case, we should expect pitch to increase with loudness, since the center of such a band must be displaced towards the positions for higher frequencies, as the overloading harmonics become more and more pronounced. This phenomenon has not been noted.

[10]Hartridge, H. A vindication of the resonance hypothesis of audition. *Brit. J. Psychol.*, 1922, **12**, 362-382.

If the cortical mechanism were made sufficiently complex, these difficulties could probably be met, since the identity of the coexcited band is actually determinate with respect to the stimulus frequency, in some manner; so that an intelligence conversant with all of the laws which are involved could compute the simple conditions for a given complicated array of effects. However, there are further troubles. Wegel and Lane have reduced the place theory to very definite form, by actually plotting the positions of the various audible frequencies on the basilar membrane, using Knudsen's pitch discrimination data as a guide. They assume that just noticeable pitch differences correspond with equal increments of distance along the membrane, and arrive at the astonishing result that the space discrimination of the auditory nerve is 0.02 millimeters, or represents a sensibility about one hundred times as great as that of touch at the finger-tips. Now there is nothing inherently absurd about such a relative figure, but, when we consider the demonstrated bluntness of the resonance curves upon the membrane, it is impossible to avoid some doubts concerning this treatment of pitch discrimination as a purely spatial affair. In effect, we are asked to believe that, in spite of the confusion of fibers which is involved, the brain can detect a displacement of the total ensemble, by an absolute amount of between one and nine fibers. (Knudsen's data, as treated by Wegel and Lane, yield the value nine, while the older discussions indicate that the number of discriminable pitches approximates the total number of auditory nerve elements: 11,000/14,000.) When we consider the lack of sharpness in the basilary images, this achievement is far in excess of the capacities of the retina or the skin.

But we still have to face the question as to how the cortex translates average nerve identity into a graded scale of qualities, or their neurological correlates. Probably the majority of modern, as well as ancient, thinkers remain quite content to relegate this problem to the limbo of metaphysics, and to agree with Johannes Müller that "the nature of this state of the nerves whereby they see light, hear sound, the nature of sound as a property of the auditory nerve, of light as a property of the optic nerve, of taste, smell, and touch, remain eternally unknown like the final causes in natural philosophy." However, if there actually are any properties of specific nerves which correspond with these things, there seems to be no good reason why they cannot be demonstrated. And if the stimulation of the successive fibers of the basilar membrane yields a continuously varying series of qualities, it seems that some feature of the brain process—in addition to mere position or identity—must vary in this way; so that the cortex must translate position back into something which has an absolute, and not merely a relative character.

In the face of all of these complexities, what an intellectual simplification it would be if we should suppose that the actual audio-frequencies are transmitted to the brain, and that they are directly responsible there for the attribute of pitch! Of course, it will undoubtedly be necessary to assume that there are other cerebral conditions for the auditory character of the experience. Otherwise, similar frequencies transmitted along the tactual nerves should yield tonal experiences. It will also be necessary to suppose that the cortex possesses mechanisms of discrimination, which permit it to deal more or less separately with frequency, the total number of impulses at the same frequency, the distribution of the impulses among the separate fibers, etc. But discrimination is one of the fundamental functions of the cortex. The process of tonal analysis, however, does not seem to fall into the category of discrimination. I quite agree with Wilkinson, in his criticism of Boring, that modern neurological ideas are not favorable to the idea that *complex* acoustic vibrations can be transformed into sequences of nerve pulses and then be sorted out again, adequately, in the brain. But this does not mean that the outstanding rhythms of the vibrations are not represented in the afferent currents; and analysis is unnecessary at the cortex because it has already been accomplished in the cochlea, in so far as it is possible at all.

I am arguing, therefore, for what may at first sight appear to be a hybrid doctrine: a resonance theory combined with a frequency theory. There may be a very considerable amount of analysis upon the basilar membrane, but it by no means follows from this that the spatial loci which are established by this action are determinative of pitch. Of course, I expect under these circumstances to encounter the question: "Then, of what use is the resonance?" And, in response to this, I may propose the counter question: "Of what use is analysis?" Tonal analysis of the type which is involved in the majority of discussions of auditory theory is a relatively uncommon process. Untrained individuals perceive clangs, chords, and the like as unified qualitative entities, rather than as mixtures. Yet I am forced to agree, again, with Wilkinson that the highly specialized structure of the cochlear organ is not only adapted to tonal separation, but must subserve a definite biological utility on this account. Is it conceivable that this utility should lie either in the domain of music or of psychological research? I think not.

The solution of this problem may proceed along a number of different lines. There is one among the latter which seems quite obvious, but I have not found any hint of it in the literature. Helmholtz introduced his doctrine of resonance as a basis for assigning different pitches to separate auditory fibers, but he might equally

well have adduced this hypothesis to explain the extremely high absolute *sensitivity* of the auditory response. In order to secure such sensitivity, resonance is quite necessary. If the basilar mechanism were restricted entirely to forced vibration, it would be unduly resistant to movement. An engineer, setting out to construct a system which would be maximally sensitive to a wide range of frequencies, would necessarily conclude with a series of resonators; and, after having built the device, he could employ the identity of the responding units as indices of frequency, or not, according to the necessities of the given situation. Thus, we may calibrate a radio tuner dial so that the wave-length is revealed by spatial position, or we may apply some other test to the reinforced oscillatory current which is developed in the set when it is tuned to a particular signal. Thus, if the range of frequencies is sufficiently low, we may be able to inspect the wave upon an oscillograph screen. It is my view that the cortex is able to make such a direct inspection, and hence does not need to look at the dial.

However, I am fully aware that there are some reasons for objecting to this assumption for the entire range of audible frequencies, although Banister's hypothesis seems to establish it definitely for frequencies of about 500 and lower. For higher frequencies, we may be in the position of the engineer who tries to record radio-frequencies upon an ordinary oscillograph, which has too great an inertia to respond. Boring believes that "there is a reasonable possibility that the refractory period of the human acoustic fibers is as low as 0.05 sigma," but Banister and other opponents of the frequency theory hold that it is probably about forty times this value, so that the highest frequency which could be transmitted along a single fiber should be 500, instead of 20,000. Personally, I am inclined to agree with Boring. The general properties of all nerves may be qualitatively alike, but we are definitely apprized that they are not quantitatively alike. The temporal characteristics of human binaural localization by means of phase difference seem to me much more illuminating regarding the probable constants of the human auditory nerve than are direct measurements upon the phrenic nerve of a dog! The history of the minimal refractory period seems to be one of progressively decreasing length (as is quite natural); Fletcher in 1923 quotes a figure corresponding to a limiting frequency of 50; Boring, in 1926, one of 704; but in a private communication an authority in this field has recently given me an estimate of 2000! Surely, it would not impose much of a strain upon the evolution of a very special sense-organ to ask for another factor of ten.

Some light is thrown upon this problem by studies in other sense-

departments. It has long been known that vibrations up to 1552 per second[11] can be distinguished from smooth contact by touch. Recent studies by Gault[12] have raised this limit to 2600. That the vibrations are transmitted to the brain as such is indicated by the possibility of discriminating differences in frequency with an accuracy of 9%, and of recognizing various vibrational forms, such as those corresponding to vowels, consonants, and so on. If the plain tactile nerves can "hear" as well as this, we might reasonably require the auditory nerves to excel them very considerably, in limiting frequency as well as discrimination. It is a general rule that biological tissues are specialized for the particular tasks which they have to fulfil.

However, we must be careful not to make undue requirements upon the auditory mechanism. What is actually the upper limit of *pitch variation?* This is not necessarily identical with the upper limit of audibility, since it is entirely conceivable that, above a certain frequency, the pitch may remain constant. This is exactly what we should expect on a frequency theory of pitch, at the refractory phase limit, at any rate if we think in terms of single fibers. The pitch sensibility above 10,000 cycles is very low, and I believe that careful studies are necessary to make it certain that the changes which are noted are really in pitch, and not in loudness, timbre, or ratio of auditory to tactile impression. The studies of Knudsen[13] appear to be the most reliable ones dealing with pitch discrimination, and they do not extend beyond 4800 cycles. He notes that when the stimulus is not applied by ear-phones, the forms of the acoustic interference patterns surrounding the head have a very important influence upon frequency discrimination. The latter is usually improved to a remarkable degree under these conditions, as a consequence of the concomitant changes in intensity and timbre. Sometimes, an actual increase in frequency may produce a decrease in pitch. Effects of this sort will naturally be at a maximum for very high frequencies.

But there is still another way of attempting to save the frequency theory of pitch, which is suggested to me, again inadvertently, by one of its opponents, Professor Banister. Let us admit, for the sake of argument, that the refractory period of the auditory fibers is such that a frequency higher than a thousand cycles per second cannot

[11]Sherrington, C. S. Cutaneous sensations. Article in Schäfer's Text-Book of Physiology, II, 1900, 924.

[12]Gault, R. H. "Hearing" through the sense organs of touch and vibration. *J. Franklin Instit.,* 1927, **204**, 329-358.

[13]Knudsen, V. O. The sensibility of the ear to small differences in intensity and frequency. *Phys. Rev.,* 1923, **21**, 84-102.

be carried upon an individual fiber. We are not, on this account, necessarily restrained from imagining that a much higher frequency can be represented by the impulses which are present simultaneously in a *number* of cooperating fibers, and we have already seen that more than one fiber must ordinarily be involved. In his experiments upon the sterno-cutaneous nerve of the frog, Adrian has observed spacings of individual impulses which are closer than would be permitted by the refractory period. He explains this effect as being due to successive impulses in separate fibers.[14] His figures show that the peaks of the action-current waves are very sharp as compared with the total time or distance which is allotted to a single impulse, so that such super-refractory frequencies can be represented distinctly up to values much higher than those which can be carried by one fiber alone.

The manner of arousal of such frequencies in multifibral conductors may be conceived as follows. We assume, with Banister, that the individual neural pulses are set off in constant phase relationships to the vibratory cycles of the basilar membrane, although disagreeing with his idea that more than one pulse may be aroused during any single cycle. Owing to variations in excitability among the hair-cells, the first cycle may set off only a fraction of the total number of cells which are mechanically affected. However, the initial impact is not without effect upon the remaining cells, since, in accordance with the general principle of the summation of stimuli, we should expect it to lower their thresholds for closely subsequent impacts. Consequently, at the second cycle, we should expect further hair-cells to be set off. Those which were aroused during the first cycle will not respond on account of their refractory condition. When a third cycle arrives, it should excite still further cells which have, characteristically, even lower thresholds but which have been primed by two preliminary impacts. This process continues, and after the lapse of an appropriate amount of time the fibers which were initially excited will again be in a position to respond. They may possibly be picked up during the later portions of their relative refractory periods, if their natural thresholds are sufficiently low. If there is any tendency toward an "overshooting" of excitability after recovery, the cells which are characteristically of low threshold may be brought up to a condition where their excitabilities intersect those of more sensitive cells, as the latter are recovering, such intersections being at a point where excitation is possible. In this way, it is conceivable that after a short period of exercise all of the cells should be about equally amenable to the stimulus, except for tempor-

[14]Adrian, E. D., *loc. cit.*, 55.

ary refractory states. However, they will be in the excitable condition at different times, irregularly, so that each cycle will set off a group of synchronized impulses.

According to this view, the psychophysiology of pitch and loudness comes out somewhat as follows. Pitch is determined by the frequency of a series of regularly spaced impulses, carried in a group of cooperating auditory nerve fibers. Loudness depends upon the total number of pulses, per second, regardless of the identity of the fibers in which they are transmitted, and without reference to frequency, except in so far as frequency is one factor in determining the number. In practically all cases, the pitch impression must rest upon some degree of synchronism between the pulsatory currents in associated fibers. At low frequencies this synchronism may well be quite perfect, owing to the relatively large time interval and the lack of necessity for splitting the frequency between statistically separate fiber groups. When the frequency exceeds the refractory limit, synchronism exists only between the simultaneously responding fibers, but there is still a rhythmic regularity of conduction in the total fiber group. On account of the shortening of the intervals of time, as well as because of a reduction in the proportion of fibers which are simultaneously effective, we might expect the tonality or "pitchiness" of the higher tones to be inferior to that of the lower ones. This is in harmony with the introspective facts. Tonality, as thus defined, would be proportioned to the degree of regularity, or evenness of spacing, of the pulses which are transmitted along cooperating fibers. Such regularity can be conceived as varying from perfection, continuously, towards a condition of complete chaos; but it is improbable that either of these extremes will actually be realized.

The exact manner in which the cortex distinguishes between the dominant nerve pulse frequency in the fiber group, and the total number of pulses per unit time, can only be conjectured. We may suppose that, regarded as a qualitative attribute of consciousness, pitch is directly correlated with frequency, provided the latter occurs in an appropriate neurological substance. An exactly similar frequency in a different substance, such as that which is attached to the tactual fibers, may yield quite a different quality. On the other hand, it is conceivable that there should be accessory auditory cells which are syntonized to specific and different frequencies, in accordance with Lapicque's principle of chronaxy, and that it is the response of such accessory cells which directly underlies the pitch experience. This idea may seem to introduce a mechanism of resonance or analysis in the cortex, which must be admitted in a certain sense. However, such syntonic response to different frequencies of regular pulsation is quite a different thing from the harmonic analysis of unevenly spaced

nerve pulses, which must be assumed as representations of complex sound waves, if we follow auditory theories like that of Rutherford. It seems improbable that the principle of syntony could effect a harmonic resolution of such an irregular sequence of excitations. We should still have to look to the distribution of frequencies among separate fiber groups for an explanation of whatever degree of harmonic analysis is actually demonstrable. This distribution also facilitates the discrimination of pitch differences, although, in accordance with the view which we are here advocating, it is not the essential basis of the latter. It is quite possible that judgments of "absolute pitch" are based upon a space criterion; but pitch discrimination is a different kind of process. This must rest upon the ability of the cortex, not to identify a certain vibration rate, but to respond specifically to a minimal difference between such rates. Such discriminatory response can almost certainly be explained in terms of the principle of syntony. In so far as there is no actual discrimination, either introspectively or in terms of behavior, it suffices to permit the cortical frequency to determine pitch directly, without the introduction of any accessory mechanisms. We do not have to assume that any operation must be performed upon the frequency in order that it should yield a characteristic form of consciousness.

In the case of loudness, we shall probably have to appeal to a cortical mechanism of summation and diffusion. If successive nerve impulses cannot develop some sort of cumulative condition in the centers, it is difficult to understand how their rate of arrival can determine characteristic brain states and the accompanying attributes of consciousness. This is a general requirement with regard to sensory intensity in all fields, which, I believe, is now quite familiar to psychologists and physiologists. If we suppose that such a process of accumulation actually exists, then the surface or volume density of this condition can be taken as the correlate of loudness in the acoustic field. The spatial dimension is necessarily introduced, not only because the cortex is spatially extended, but because of the cooperation of a plurality of conducting fibers in practically all cases of sensory response. These fibers must deliver their impulses in spatially distinct, although adjacent and connected, regions in the cortex.

The above-advocated doctrine regarding the neurological basis of pitch would seem to be amenable to test, in opposition to the "place" theory, by observations such as the following. It is a familiar fact that individual audibility curves frequently show rather sharp dips, indicative of relative deafness to restricted ranges of frequencies. However, experiment shows that if the intensity of the stimulus is made sufficiently great, any frequency within such a range can

become audible.[15] This is explained by supposing that with increasing intensity the vibration spreads along the basilar membrane until it impinges upon a section which is in a normal state of sensibility. In this case, we presumably have the original frequency operating at an abnormal "place" on the membrane. The frequency of the nerve pulses should be that of the stimulus, and the frequency theory demands that the resulting pitch should be the same as if the manner of response were that of a normal ear. The "place" theory, on the other hand, requires a change in the pitch. Some difficulty might be encountered in determining whether the pitch had changed, if it were not for the fact that defects of this sort are ordinarily monaural. I am informed by a leading authority in acoustic research that he has made observations of this sort, under conditions favorable to a crucial test, and that there is no change in pitch. The exact details of these conditions may require quantitative study before we can be entirely sure of the finality of our conclusions, but the indications are strongly opposed to a "place" interpretation.

## V. The Psycho-Neurology of Volume, Localization, and Brightness

The assumptions which we have already made enable us to deal with the psychophysiology of *volume* in a very satisfactory manner. Let us agree with Boring that the cortical correlate of volume consists in the area, or spatial extent, of that portion of the cortex which receives all of the impulses derived from a single kind of vibration upon the basilar membrane. We may consider, firstly, the case of pure tones. It should be clear from the foregoing discussion that, for equal loudnesses among such tones, a larger number of fibers must cooperate at low than at high frequencies. In fact, on the simplest assumptions, the number of fibers should be inversely proportional to the frequency. It is therefore to be expected that the cortical areas will be smaller for high than for low pitched tones, at the same loudness. This corresponds with the experimental fact that volume decreases regularly as pitch advances. According to Rich, the volume is proportional to the logarithm of the frequency, but the exact form of the law can be derived theoretically only on the basis of quantitative assumptions regarding the psychophysiological relationships which are here concerned. Offhand, we have no reason for asserting that introspectively just noticeable increments of volume involve the addition of equal numbers of cooperating fibers. It is entirely possible that the cortical discrimination mechanism, which is operative in this process, responds to equal fractional increments

[15] Fletcher, H., *loc. cit.*

in the total number of fibers or in the area of the corresponding region of cerebral activity. Again, the discrimination apparatus may deal with a linear function of the active cortical area, whereas the number of fibers determines a surface measure.

It will be noted that the increase of volume with loudness is also accounted for by the above analysis.[16] Any augmentation of the amplitude of vibration of the basilar membrane means a more or less symmetrical spreading of the excitation along its length. More nerve fibers are thus involved in the synergic activity. This brings about an increase in the total number of nerve pulses which are transmitted to the cortex, per second, thus raising the loudness value, and at the same time the volume must also increase. With regard to the afferent nerve, volume rests upon the total number of cooperating fibers, while loudness is associated with the number of pulses transmitted through a cross-section of the fiber group per unit of time.

These considerations leave us with some uncertainties regarding the exact functional relationships which should hold between volume, pitch, and loudness. Boring suggests that the logarithmic dependence of volume upon frequency which was found by Rich[17] was artifactual, in consequence of the latter's failure to control his relative loudnesses. On the other hand, it is common experience that low pitches in general are more voluminous than are high pitches. Halverson found that the laws relating volume and loudness to intensity are different in form. Experiments are obviously to be desired in which tones of really equal loudness, but differing in pitch, are compared with respect to volume. A thorough study of this system of relationships should throw light, not only upon the afferent mechanisms, but also upon those of the cortex.

The relationship between volume and localization, or phase difference between the two ears, which has been the subject of speculation by Boring, seems to me to be much less amenable to fruitful analysis in the present state of our knowledge. It seems reasonable to suppose that the fibers, or nerve currents, from corresponding points in the two basilar membranes are brought into approximate juxtaposition in the cortex. The relationship would thus be analogous to that which is supposed to exist for the two retinas. Such union of corresponding sets of nerve impulses would permit a comparison or interaction, the outcome of which could be determined by the transmitted phase differences. On the assumption that the combination of the right and left auditory currents follows the same

[16]Cf. Boring, E. G., *loc. cit.*, 162-163.

[17]Rich, G. J. A study of tonal attributes. *Amer. J. Psychol.*, 1919, **30**, 121-164.

principles as those applying to adjacent currents from the same ear, we should expect binaural tones to be both louder and more voluminous than are monaural ones. The increase in loudness can be demonstrated very readily by means of a pair of head telephones and a radio receiving set, but the volume judgment is more difficult to make. Knudsen[18] finds a very considerable increase in the fineness of pitch discrimination by binaural as compared with monaural reception.

However, it is almost certain that the neurological principles which are involved in binaural combination are different from those which characterize the relationship between impulses from a single ear. Just what we should expect to happen as a consequence of variation in phase difference must depend upon our conception as to the manner in which the two groups of impulses combine. If the monaural and binaural combinations followed exactly the same laws, we should expect to find changes in pitch and general quality, with changes in phase difference. Suppose, for example, that the latter is such that the impulses from one ear unite with those from the other to form a total process which has a regular periodicity. Then the frequency of this process will be double that for a single auditory nerve, and the pitch should correspond to an octave of the actual stimulus. Evidently, then, if the frequency theory of pitch is to hold, the neural connections between the two auditory nerves, or with the higher centers, must conform to a different scheme from that which characterizes the relationships between fibers of the same nerve. I experience some difficulty in following Boring's arguments regarding the relationship between phase difference and volume, but I agree that a careful study of these relationships should assist us in determining the nature of the cerebral connections which operate between the two auditory nerves.

The attribute of *brightness* is said to be so closely correlated with that of pitch that the two form a single dimension, for musical tones. However, this correlation is broken down when we deal with auditory qualities which depart from the purely tonal type. Thus, the vowels have characteristic pitches but their brightnesses are less than those of the corresponding tones. Ogden,[19] in a somewhat symbolic manner, identifies brightness with the saliency of the dominant pitch in a "tonal manifold." Low pitches are blunt or dull, whereas high ones are sharp or peaked. Similarly, vowels are blunter than are tones of the same pitch. Ogden's diagrams involve a confusion of the volume and pitch dimensions, but they express a real

[18]Knudsen, V. O., *loc. cit.*, 98-99.

[19]Ogden, R. M. Hearing. New York: Harcourt, Brace, 1924. See pp. 69-75.

feature of auditory experience and can be given a definite meaning in terms of the assumptions of the present paper.

Now, if we consider the response of the basilar membrane, as it is depicted by Wegel and Lane,[20] we see what this interpretation must be for the activity of the sense-organ. The vibration with a low frequency stimulus not only covers a longer stretch of the membrane, but it has a much blunter distribution of amplitudes than have vibration patterns corresponding to higher frequencies. The general proposition holds that the higher the frequency the sharper is the resonance and the more restricted is the band of the membrane which is being used. From the standpoint of the sense-organ, we might regard the degree of sharpness of the response, the saliency of the maximum, as the correlate of brightness. It is clear that this saliency increases regularly with pitch, as required by introspective data.

However, we have still to face the question as to how such saliency can be represented in the auditory nerve impulses. If the fibers respond in accordance with the all-or-none principle, it would seem that the amplitudes of their activities must all be the same, regardless of radical differences between those of corresponding portions of the basilar membrane. We have already noted this as a difficulty to be met by the view of Fletcher that the point of maximal response determines pitch in accordance with a "place theory." However, in addition to the possibilities which we have previously mentioned, there is another which might be utilized by advocates of the place type of theory although, personally, I do not regard it as either necessary or adequate to the explanation of pitch. It may, however, be applied at least tentatively to an explanation of brightness.

If we reason in harmony with general physiological principles, we shall be led to doubt the complete accuracy of Banister's assumption that the nerve pulses in the auditory fibers are set off in constant relation to the *phase* of the basilar vibrations. It would seem that the exact phase position at which an impulse is liberated should depend upon the *degree of local displacement* of the membrane, rather than upon the phase, per se. At points where the amplitude of motion is greater, impulses should appear at an earlier point in the vibration cycle than where the amplitude is lower. That portion of the membrane which has the greatest amplitude of movement should, therefore, set off impulses sooner than any other portion. Consequently, there should be a sort of *curvature of the neural wave-front* within the group of fibers whose activities correspond to the given excited band of hair-cells. It is an obvious suggestion that the bright-

[20]Wegel, R. L., & Lane, C. E., *loc. cit.*, 281.

ness increases with the degree of such wave-front curvature, or the saliency—either in time or space—of one portion of the impulse group, as compared with other portions. This saliency is not in the frequency-pitch dimension, as Ogden seems to conceive it, but is in the allied dimension of analytical separation. It is also representative of form within the dimension corresponding to volume. The increasing sharpness of resonance towards the higher frequencies must be represented in the corresponding neural wave-fronts so that brightness should increase regularly with advancing pitch, in harmony with the facts. Vowels, however, have a blunter distribution of amplitudes upon the membrane, and hence in the nerve conduction process.

We must note that this modification of Banister's hypothesis does not rob the latter of its explanatory value for localization in terms of phase difference. The curvature of the neural wave-front will be the same for both ears, so that the phase relationships between impulses in corresponding fibers remain unaltered. Any attempt to apply this wave-front curvature principle to the explanation of pitch, in accordance with a place theory, seems to me to be doomed to failure, because of the general lack of sharpness of the basilar resonance, as compared with the fineness of pitch discrimination. Brightness is a much vaguer sort of attribute than is pitch, and if it were not closely correlated with the latter, it would probably show a very high differential threshold.

## VI. The Psycho-Neurology of Non-Tonal Auditory Qualities

Relatively few theories of audition have paid adequate attention to the problem of the physiological basis of noises, vocables, and other auditory experiences not lying in the strictly musical category. The general view which has been outlined above enables us to deal with these experiences and their interrelationships in a rather satisfactory manner. Incidentally, we are also able to explain why it is that these qualities cannot be analyzed introspectively into definite constituents.

It is evident that the capacity of the cochlear mechanism for bringing about a spatial separation of acoustic frequencies upon the basilar membrane is rather limited. Let us consider a case in which only two frequencies are present, and for the time being we may neglect their subjective harmonics. Each of these frequencies involves a considerable number of adjacent nerve fibers, and excites the latter in a rhythmic manner. One group of fibers transmits impulses at one frequency, and another separated group, at a different frequency. However, let us see what must happen when these fre-

quencies are brought more closely together. With such change, a point must be reached at which there is an overlapping or interference of their action upon the nerve fibers. Under these conditions, there may still be two groups of fibers which are transmitting regular frequencies to the brain, but there will be an intermediate group which will be subject to temporally irregular stimulation. The pattern of the latter will correspond to the manner of combination or interference of the two frequencies. When the frequencies are quite close together, the irregularity of the current in the intermediate band of fibers may not be very marked, and the width of this band will tend to vary rhythmically at a rate equal to the difference between the frequencies in question. This is due, of course, to the alternate summation and inter-cancellation of the two vibrations, in accordance with the physical principle of "beats." The frequency of the impulses in the overlap band should be intermediate between that of the two interfering frequencies, a fact which can be appreciated by inspecting a diagram showing the form of combination of two waves which are in a beating relationship.[21] The overlap band, with its characteristic impulse frequency, will obviously correspond to the so-called subjective *intermediate tone,* which "carries the beats" in consciousness. The subjective beats are referable to the rhythmic variations in the width of the overlap band, which naturally yield similar changes in loudness. There should be no beating of the concomitant primary tones, since the physical frequencies are substantially separated on the portions of the membrane corresponding to these experiences. However, if the frequency difference is very slight, there may be practically a complete coincidence between the two excited bands, with no appreciable irregularity, but merely a waxing and waning of the membranal amplitudes. In this case there should be no intermediate tone, the beats being carried by the single original tonal impression.

When the beating notes are separated by very considerable frequency differences, the irregularity of the pulsatory currents in the intermediate group of fibers will become very marked. Experiments show that the corresponding auditory experiences assume a noisy character. Now, it seems to me reasonable to suppose that *noisiness,* or any other qualitative departure from tonality, should be attributed to *irregularity* or unequal spacing of the successive pulses in the auditory fibers. Such irregularity will occur when the peaks of a compound, or non-sinusoidal, vibration, at a given point on the basilar membrane, become sufficiently salient so that each one

[21]See, e.g.: Luciani, L. Human physiology. (English trans.) London, 1917, Vol. IV, 232.

of them arouses a nerve impulse. Such a non-rhythmic neural current cannot be expected to duplicate, exactly, the form of the corresponding mechanical vibration. If certain components of the latter are of very low relative amplitude or are lacking in saliency, they will not be represented at all in the nerve currents. However, compound wave-forms can readily be produced to have an irregular distribution of salient peaks. The existence of such irregularities in the original sound which impinges upon the tympanum does not necessarily mean that they will still exist at particular points upon the basilar membrane, on account of the spatial analysis which occurs on the latter. However, if the component frequencies are crowded closely together, the cochlear reasonance may be unable to separate them completely, so that to this extent there will be failure of analysis, and at least some of the nerve fibers will be irregularly excited.

Now, the probability of such overlapping of basilar excitation, with consequent departure from rhythmicity in the corresponding nerve currents, is increased tremendously by the presence of harmonics and combination frequencies that are introduced by the non-linear response of the ear itself. These distortion products require that even with a pure sinusoidal stimulus, at reasonably high intensity, a very considerable number of nerve fiber groups will be excited. The frequencies which are characteristic of these groups will naturally be more or less harmonically related to one another. If, now, we introduce a second frequency, which does not coincide with any of the frequencies which are already present on the membrane, there will tend to be interference, not only between the fundamentals, but also between the higher harmonics or combination tones. Fletcher has shown that if a group of frequencies, which are separated by constant differences (originally harmonic), is raised or lowered arithmetically, so they are no longer expressible as integral multiples of the lowest one, the corresponding experience loses all musical quality and becomes noisy.[22] This is to be attributed to the fact that when the relationships are not of a harmonic character, each component in the combination gives rise to its own peculiar harmonics; and, therefore, a very complex assembly of difference and summation frequencies arises from the entire system, so that the isolation of individual regular frequencies upon the membrane becomes impossible. Hence many diverse irregular sequences of impulses will be transmitted along the nerve fibers in separate sections of the auditory conducting system.

Now, a *noise,* objectively, has this character of great complexity

[22]Fletcher, H. The physical criterion for determining the pitch of a musical tone. *Phys. Rev.,* 2nd ser., 1924, **23**, 427-438.

in its acoustic spectrum, together with a lack of integral or harmonic relationships between its component frequencies. Many noises have a practically continuous spectrum. This means that a clear-cut isolation of regular vibrations upon the basilar membrane is impossible. However, the distribution of energy with respect to frequency, in a noise, usually shows characteristic maxima, so that the noisy vibrations may be distributed in definite ways over the membrane, and the resulting neural currents will show some degree of frequency qualification. Although irregular, they may be characterized by close spacing or, on the contrary, they may be separated from each other rather widely. Consequently, we should expect the corresponding experiences to have pitch qualities which are determined by the general frequency tendencies of the impulses, but to be lacking in true tonality in proportion to the given degree of the irregularity. It is, of course, obvious that any amount of irregularity in the activity of one portion of the basilar system is quite consistent with perfect regularity in some other sector. Hence, we may have concomitant tones and noises, which are analytically separable by introspection. Also, high and low pitch noise groups may be subject to similar treatment, because they may achieve isolated representations in the nerve currents. Vocables, including vowels and consonants, involve complex patterns of this sort. The vowels are less noisy, more tonal, than are the consonants, corresponding to a greater degree of regularity in their correlated nervous processes.

We are thus presented with a picture of the neurological basis of total auditory quality which may be described somewhat as follows. The untutored cerebral cortex does very little analyzing, in a musical or psychological sense. The auditory experience, at any moment, is correlated with the total activity of all of the nerve fibers on both sides of the head. However, this neurological system presents a number of at least partially independent variables, which are individually correlated with separable attributes of auditory experience. The spatial separation of different frequency components, and their assignment to distinct groups of nerve fibers, in the case of a musical stimulus, increases the ability of the system to distinguish stimulus differences, particularly when the latter have complicated frequency distributions. The possible variety of the correlated experiences is increased, correspondingly. However, such differences are ordinarily given in consciousness as qualitative rather than as structural. A special introspective attitude is required in order to introduce the latter feature to any marked extent. We may suppose that this attitude involves the establishment of more or less insulated connections to local groups of fibers.

When the stimulus is very complex, as is usually the case under

natural circumstances, there will be a characteristic pattern of regular and irregular nerve currents in the auditory field, the exact form of this pattern being specifically indicative of the constitution of the stimulus which is operative. In proportion as the components of this pattern consist in regular neural impulse streams, the result will be musical, even if it does not comprise a single pure tone. In such a musical combination, the pitch appears to be determined primarily by the *lowest* of all of the frequencies which are present. When we are dealing with a stimulus which has objective harmonics, the fundamental is very strongly reinforced within the ear by the generation of difference tones between these harmonics. However, if we have a pure sinusoidal stimulus, and if the intensity is sufficiently low to avoid subjective harmonics, the pitch must be determined by the single isolated nerve impulse frequency. When a complex musical experience is analyzed, introspectively, into a number of concomitant pitches, each component frequency corresponds to a particular one of these separated pitches.

When the vibration of the basilar membrane becomes so complex as to introduce overlapping nerve excitations, due to different frequencies, an element of irregularity appears in the nerve process, and this is characterized not only by the time pattern of the irregularity in question, but by its position within the total nerve fiber field. It may be variable in degree, in position, and in proximity or separation from concomitant regular processes, etc. Thus, for each stimulus, there will be a spatially and temporally configured synergy in the auditory field of the cortex, having dimensions and general properties which seem to make it a suitable correlate for auditory experience, as we find the latter.

It is not necessary to assume that the correlations between consciousness and particular aspects of the afferent process are completedly rigid. The cortex shows considerable flexibility in its utilization of significant sensory data. Thus, as we have noted, our judgment of pitch refers to the frequency of the fundamental in the case of a clang, but this judgment becomes more complex when the experience is analyzed. Timbre corresponds to the distribution of regular frequencies over the auditory field or to the particular combination of frequencies, considered merely as temporal patterns, regardless of spatial position. However, general pitch impressions may sometimes be determined by position in the field. In other words, it is not necessary to rule out the place theory entirely.

It may be noted that the above discussion of the mechanism of the ear, and the relationship of the resulting neural frequency patterns to consciousness, removes the difficulties which have hitherto been raised against the Helmholtzian explanation of consonance and

dissonance.[23] The existence of subjective harmonics means that beats and noise can appear in consequence of interference between the distortion over and combination tones, even when the stimuli are purely sinusoidal, if they themselves stand in inharmonic relations to each other. Moreover, our ordinary concept of a pure tone must naturally be adulterated with that of its harmonics, as subjectively generated. It is not necessary to appeal, as Ogden does in a similar argument, to the familar presence of such harmonics in objective musical sounds. The octave provides the most perfect consonance, because it corresponds with the subjective harmonic which is represented most loudly. At the same time, the difference tone which it produces, in combination with the fundamental, and also with the third harmonic, is identical with the fundamental in frequency. Similar principles apply, to a lesser degree, to the third, fifth, and higher harmonics, which become less consonant in the order stated. Octave quality can be accounted for also on the supposition that our concept of a pure tone is adulterated with that of its normal subjective harmonics. In this manner each tone contains its own octave in a prominent manner.

## VII. Summary

I trust that the foregoing speculative discussion will not be interpreted as the formulation of "a new theory of audition," since the purpose of the present paper is merely to point out certain theoretical possibilities which seem not to have been duly stressed. The views which are advocated here must necessarily be regarded as subject to modification on the basis of future researches or facts which have escaped my notice. However, the views in question may be summarized briefly as follows.

The notion of the mechanics of the inner ear which was first suggested by Roaf, and which has been developed by members of the staff of the Bell Telephone Laboratories, may be accepted as generally satisfactory. However, the neurological bases of the attributes of auditory experience require further elucidation. Different acoustic frequencies are actually assigned to separate nerve fiber groups, as contemplated by Helmholtz, but pitch is nevertheless determined by the nerve impulse frequency and not by the anatomical identity of the fibers. Difficulties due to limitation by refractory phase are removed by partitioning the conduction of the total frequency among the cooperating fibers, when the frequency is excessively high. Loudness depends upon the total number of impulses passing a fixed cross-section, per second, in a group of cooperating fibers, while volume is

[23] Cf. Ogden, R. M., *loc. cit.*, 130 and 140-149.

related to the mere number of such fibers. Localization rests primarily upon the time difference in the brain between corresponding sets of impulses from the respective ears. Auditory brightness is due to the sharpness of the impulse "wave-front" within the fiber group. The departure of the auditory experience from the purely tonal or musical type is due to the production of irregularity in the timing of the nerve impulses, in consequence of effective overlapping of the action of different frequencies on the basilar membrane. The total unanalyzed auditory quality is determined by a complicated pattern, or Gestalt, involving both the spatial and temporal distributions of neural currents which vary in frequency and also in degree of rhythmicity.

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## LA PSYCHOPHYSIOLOGIE DES QUALITÉS ET DES ATTRIBUTS AUDITIFS

(Résumé)

L'idée de la méchanique de l'oreille interne suggérée d'abord par Roaf, et développée par les membres des laboratoires Bell Telephone, peut être considérée en général satisfaisante. Cependant, les bases neuralogiques des attributs de l'expérience auditive exigent encore un éclaircissement. De différentes fréquences acoustiques sont actuellement données aux groupes séparés des fibres des nerfs, selon l'idée de Helmholtz, mais la hauteur est déterminée cependant par la fréquence des impulsions des nerfs et non pas par l'identité anatomique des fibres. Les difficultés dues à la limitation par des phases réfractaires sont enlevées au moyen de partager la conduction de la fréquence totale entre les fibres coopérants, quand la fréquence est excessivement élevée. La force du ton dépend du nombre total d'impulsions passant devant une section fixée, chaque seconde, dans un groupe de fibres coopérants, tandis que le volume a rapport seulement au nombre de tels fibres. La localisation dépend surtout de la différence de temps dans le cerveau entre les groupes correspondants des impulsions des oreilles respectives. La clarté auditive est due à l'intensité du "front des ondes" des impulsions dans le groupe de fibres. Le départ de l'expérience auditive du type simplement tonal ou musical est attribué à la production de l'irrégularité du temps où viennent les impulsions des nerfs, par conséquent d'un empiètement effectif de l'action des différentes fréquences sur la membrane basilaire. La qualité auditive totale non analysée est déterminée par un patron compliqué, ou Gestalt, comprenant les distributions spatiales et temporales des courants neuraux qui varient en fréquence et aussi en degré de rhythme.

TROLAND

## DIE PSYCHOPHYSIOLOGIE DER EIGENSCHAFTEN UND ATTRIBUTE DES GEHÖRS

(Referat)

Die Vorstellung des Mechanismus des innern Ohres, die zuerst von Roaf angedeuter und im Bell Telephon Laboratorium weiter ausgestaltet wurde, kann wohl im ganzen als befriedigend angesehen, werden. Die neuro-

logischen Basen der Attribute der auditorischen Erfahrungen, jedoch, bedürfen weiterer Erläuterung. Verschiedene akustische Häufigkeiten werden tatsächlich gewissen Nervenfasergruppen zugeschrieben, wie Helmholtz vermutete; aber Tonhöhe wird dennoch durch die Häufigkeit des Nervenimpulses bestimmt, und nicht durch die anatomische Identität der Fasern. Schwierigkeiten, die durch die Beschränkung der abwehrenden (refractory) Phase verursacht werden, beseitigt man durch die Verteilung der Konduktion der Totalhäufigkeit unter die zusammenwirkenden Fasern, wenn die Häufigkeit ungewöhnlich hoch ist. Die Stärke des Tones hängt von der Gesammtzahl der Impulse ab, welche einen bestimmten Querschnitt einer zusammenwirkenden Fasergruppe per Sekunde durchkreuzen, wogegen die Tonfülle (Volumen) von der Anzahl der Fasern abhängt. Lokalisation wird hauptsäschlich von dem Zeitunterschied zwischen den entsprechenden Impulsgruppen des betreffenden Ohres bestimmt. Auditorische Helligkeit oder Klarheit des Tones wird durch die Stärke der "Wave-front" des Impulses innerhalb der Fasergruppe bestimmt. Die Abweichung der auditorischen Erfahrung von dem rein tonalen oder musikalischen Typ wird wohl durch eine Unregelmässigkeit in der Zeitanpassung des Nervenimpulses veranlasst, in Folge des wirksamen Übereinandergreifens der Aktion verschiedner Häufigkeiten auf der Grundmembrane. Die unanalysirte, auditorische Gesammtqualität wird durch ein komplizirtes Muster bestimmt, das die räumliche sowohl wie die zeitliche Verteilung des Nervenstromes einschliesst, welche in der Häufigkeit und daher auch im Grad des Rhythmus (rhythmicity) varrieren.

TROLAND

## ПСИХОФИЗИОЛОГИЯ СЛУХОВЫХ ОЩУЩЕНИЙ.

### (Реферат).

Представление о механизмах внутреннего уха, выдвинутое впервые Roaf'ом и разработанное впоследствии рядом сотрудников Белевских телефонных лабораторий, может быть в общем признано удовлетворительным. Тем не менее неврологическая основа слуховых ощущений требует дальнейшего изучения. Различные акустические частоты указывают, несомненно, на наличие раздельных групп нервных волокон, как это полагал уже Гельмгольц; однако, высота тона определяется частотой первых импульсов, а не анатомическим тождеством волокон. Затруднения, вызываемые ограничением, которое создается рефрактерной фазой, устраняются распределением проведения всей гаммы частот между сотрудничающими волокнами, когда частота становится слишком высокой. Сила звука зависит от общего числа импульсов, проходящих в секунду, через определенный поперечный разрез группы сотрудничающих волокон, в то время как об'ем определяется лишь числом этих волокон. Локализация основана прежде всего на различиях во времени прохождения через головной мозг корреспондирующих групп импульсов от соответствующих ушей. Отчетливость слуховых ощущений зависит от остроты импульса в пределах данной группы волокон. Отделение слуховых ощущений от ощущений чисто тоновых или музыкальных является результатом неравномерности в чередовании нервных импульсов, иначе говоря—перекрытия действий различных частот на основной перепонке. Вся непроанализированная область слуховых ощущений определяется как сложная конфигурация или форма (Gestalt), включающая в себя пространственное и временное распределение нервных токов, которые различаются между собой как по частоте, так и по степени ритмичности.

**Троланд (Troland).**

# THE INTERPRETATION OF FACIAL EXPRESSION IN EMOTION*

*From the Psychological Laboratories of Wesleyan University*

CARNEY LANDIS†

Proof that man has great confidence in his ability to judge facial expressions correctly may be found every day. We often see a face which we immediately and involuntarily think of as denoting pleasure, grief, boredom, fear, etc. Despite this common belief, very little work has ben done on the subject in a scientific way. Darwin (1873) in his *Expression of the Emotions in Men and Animals* states that we have an instinctive power of recognizing expressions. In this he contradicts Lemoine who argued that man did not possess any innate knowledge of expressions. Darwin supported his belief chiefly from his own personal experience. When his child was only four months of age, Darwin noticed that he understood a smile, received pleasure from seeing a smile, and answered it by another. Also when the child was six months of age his nurse pretended to cry and immediately the child's face assumed a melancholy expression. He had never associated with other children and had never seen a grown person cry, hence Darwin concluded that it was innate feeling letting him know that the pretended crying of his nurse expressed grief and this, through the instinct of sympathy, excited grief in him.

Langfeld (1918) made use of pictures sketched by an artist of faces expressing various emotions, showing them to his subjects with the request that they state what emotion was being expressed. Langfeld concluded that certain emotional expressions could be judged successfully from pictures.

Sherman (1927) demonstrated that both trained and untrained observers could not correctly name the emotional reaction of a very young infant either from direct observation of the infant or from motion pictures. The situations employed were hunger, restraint, dropping, and sticking with a needle. Students in psychology used a range of 12 to 25 different emotions as possible causes for the reactions, medical students named 8, and nurses, 7. In brief, his work

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†Assisted by H. W. Fairchild.

indicates that unless the situation is recognized, the reaction cannot be properly labeled and when the situation is known there still remains a certain amount of difference of opinion as to the proper label for the behavior.

Ruckmick (1921), Gates (1925), and several other investigators have tried out the ability of observers to name correctly photographs which had been posed to represent one or another emotion. As a result of these studies one may say that expressions of "pleasantness" and of "surprise" are correctly labeled in a majority of cases but that there is a wide divergency of opinion and a great susceptibility to suggestion for most other posed "emotions."

As far as I can ascertain, Sherman's (1927) study is the only one so far reported in which an attempt has been made to name emotions on the basis of observation of the actual "normal emotional behavior."

## Purpose

The purpose of this experiment was to investigate the following points:

(1) Given a collection of photographs taken during emotional situations, to determine what interpretation judges will place on the "emotions" portrayed.

(2) Can one more accurately interpret the "emotion" portrayed in response to actual stimulation or when the portrait depicts the acting of that situation?

(3) Can a judge name the situation that might arouse the behavior which a portrait shows?

(4) How certain are judges of observations of this character?

## Method

Seventy-seven photographs were selected from the 844 studied and reported in a previous paper (Landis, 1924). These 77 were chosen because they were, in my opinion, "very expressive." Fifty-six were photographs which had been taken in actual situations, while 21 were photographs of subjects who were trying to reproduce their reactions to the situations as they had previously experienced them. Pictures of 11 men and 11 women were used. Each picture was trimmed so that only the head and shoulders were depicted. These pictures had been obtained in the following situations. (The reader is referred to Landis (1924) for further details.)

(1) Listening to popular music.

(2) Listening to technical music.

(3) Reading a selection from the Bible.

(4) Truth and falsehood—the subject was photographed while telling the truth and while lying.

(5) Smelling ammonia.

(6) Exploding a firecracker suddenly beneath the subject's chair.

(7) The subject was told to write a description of the meanest or most embarrassing thing he ever did, and this was then read to him by the experimenter.

(9) Showing the subject pictures of skin diseases.

(10) Mental multiplication with distraction.

(11) Seeing pornographic pictures.

(12) Seeing a set of posed photographs of feminine artist's models.

(13) Reading sex case histories.

(14) Without looking, the subject put his hand into a pail which contained 3 frogs in water and then received a strong electric shock.

(15) Cutting the head off a live white rat with a dull knife.

(16) Mental multiplication with electric shocks.

(17) Relief—the subject was told that the experiment was over. These photographs were projected on a screen by means of a projection lantern. Forty-two judges were used, all of them being students at Wesleyan University and enrolled in one or another psychology course.

Each judge was given a set of mimeographed blanks. At the head of each set appeared the following directions.

"You will be shown a series of photographs which seem to be expressive of various emotions or feelings. Consider each photograph carefully and then try to state what emotion or feeling you think the subject is experiencing. Enter this in the first column opposite the number of the picture. Then try to imagine or guess what sort of stimulation would give the particular expression photographed. Enter this in the second column. In the third column enter the per cent of certainty you feel concerning your two previous judgments."

The first column was headed "Emotion Portrayed," the second "Situation," and the third "Per Cent Certainty." The subjects were instructed that 0 per cent certain meant no certainty at all, 50 per cent moderate certainty, and 100 per cent absolute conviction.

## Data and Results

Below is given a compilation of the results of this study. The results are grouped so that all photographs originating in the same situation appear together. In the first column are the numbers of the pictures. In the second is given verbal report ("introspection") given by the subject at the end of the situation. The third column gives the emotional labels which were attached to these photographs in more than 20 per cent of the judgments. All labels occurring in less than 20 per cent of the judgments are omitted as they cover a wide "hit or miss" range and add nothing of importance. In the fourth column appears the "Average Per Cent of Certainty" which

the judges accorded each picture. Column 5 gives the situation in which the judges said such an expression would occur or be appropriate. Here as in Column 3 all judgments occurring in less than 20 per cent of the cases are omitted. The results from several pictures are omitted since the range of judgments was so wide that no 20 per cent agreement was reached.

| 1 Picture number | 2 Actual "introspection" | 3 Emotional labels and per cent occurrence | 4 Average per cent certain | 5 Situations noted which might give expression |
|---|---|---|---|---|
| *Situation 1—Popular Music* | | | | |
| 376 | None | Joy 81% | 44% | Pleasant 68% |
| 521 | Mixed feelings | Joy 72% | 39% | Pleasant 68% |
| 1515 | None | Anger 21% | 46% | Unpleasant 20%<br>Religion 36%<br>Pleasant 20% |
| 135 | Thinking of reaction | Sorrow 32% | 38% | Pleasant 20%<br>Unpleasant 30% |
| *Situation 2—Technical Music* | | | | |
| 636 | None | Fear 27%<br>Sorrow 22% | 42% | Unpleasant 66% |
| 635 | None | Motherly 58%<br>Embarrassed 21% | 43% | Pleasant 25%<br>Embarrassed 30% |
| 479 | None | Sorrow 66% | 32% | Unpleasant 36%<br>Motherly 20% |
| 596 | Quiet pleasure | Joy 20%<br>Attention 23% | 30% | Religion 36% |
| *Situation 4—Truth or Falsehood* | | | | |
| 7 | Telling truth | Joy 59%<br>Sorrow 21%<br>Motherly 21% | 31% | Unpleasant 26% |
| 433 | None | Joy 85% | 45% | Pleasant 76% |
| 718 | Lying | Devout 35% | 30% | Religion 44%<br>Unpleasant 24% |
| *Situation 5—Smelling Ammonia* | | | | |
| 103 | Not bad | Joy 74% | 35% | Pleasant 27%<br>Motherly 31% |
| 722 | Surprise | Joy 70% | 43% | Pleasant 53% |
| *Situation 6—The Firecracker* | | | | |
| 8 | None | Joy 83% | 27% | Pleasant 52% |
| 229 | Surprise | Joy 54% | 46% | Pleasant 62% |

| 1 | 2 | 3 | | 4 | 5 | |
|---|---|---|---|---|---|---|
| 267 | Surprise | Joy<br>Motherly | 47%<br>41% | 40% | Pleasant<br>Motherly | 44%<br>50% |
| 402 | Surprise | Joy | 83% | 35% | Pleasant | 74% |
| 151 | Not surprised | Joy | 85% | 51% | Pleasant | 83% |
| *Situation 7—Faux Pas* | | | | | | |
| 231 | Thinking | Attention<br>Fear | 22%<br>22% | 35% | Unpleasant<br>Religion | 28%<br>27% |
| 383 | Amused | Joy<br>Sex<br>Embarrassed | 43%<br>20%<br>20% | 33% | Pleasant | 56% |
| 694 | None | Joy | 75% | 34% | Pleasant | 73% |
| 306 | Useless to remember | Sorrow | 26% | 36% | Unpleasant | 63% |
| 536 | None | Sorrow<br>Motherly | 44%<br>29% | 36% | Motherly<br>Unpleasant | 33%<br>25% |
| 105 | Not bothered | Joy | 92% | 51% | Pleasant | 75% |
| 725 | Slight embarrassment | Anger<br>Joy<br>Motherly | 23%<br>31%<br>23% | 42% | Pleasant | 76% |
| *Situation 10—Mental Multiplication with Distraction* | | | | | | |
| 13 | None | Anger<br>Attention | 34%<br>26% | 20% | Pleasant<br>Unpleasant | 33%<br>22% |
| 15 | None | Fear<br>Surprise | 43%<br>45% | 36% | Unpleasant | 40% |
| 240 | Irritation | Sorrow<br>Devout | 23%<br>23% | 40% | Religion<br>Unpleasant | 37%<br>25% |
| 333 | Wanted to kick | Joy<br>Motherly | 36%<br>46% | 34% | Pleasant<br>Religion | 47%<br>21% |
| 513 | None | Joy<br>Motherly | 26%<br>34% | 35% | Pleasant | 58% |
| 199 | Difficult to retain numbers | Joy | 89% | 43% | Pleasant | 68% |
| 541 | Exasperation | Anger | 34% | 30% | Unpleasant | 60% |
| 160 | Exasperation | Joy | 87% | 41% | Pleasant | 71% |

| 1 | 2 | 3 | | 4 | 5 | |
|---|---|---|---|---|---|---|
| *Situation 11—Pornographic Pictures* | | | | | | |
| | "Dirty | Motherly | 23% | | | |
| 515 | men" | Joy | 66% | 40% | Pleasant | 32% |
| *Situation 12—Nude Art Studies* | | | | | | |
| 87 | Excited | Joy | 77% | 39% | Pleasant | 71% |
| | Aesthetic appreciation | | | | | |
| 581 | | Attention | 30% | 49% | Unpleasant | 31% |
| *Situation 13—Sex Case Histories* | | | | | | |
| 646 | None | Sorrow | 63% | 39% | Unpleasant | 53% |
| | | | | | Maternal | 26% |
| | | Sorrow | 28% | | | |
| 644 | None | Motherly | 21% | 46% | Unpleasant | 26% |
| | | Attention | 21% | | Maternal | 35% |
| | Mild disgust | | | | Pleasant | 30% |
| 280 | | Joy | 63% | 74% | Maternal | 21% |
| *Situation 14—The Frogs* | | | | | | |
| | | Joy | 22% | | | |
| 647 | None | Motherly | 48% | 41% | Pleasant | 46% |
| | Shock startled | Joy | 20% | | | |
| 344 | her | Motherly | 76% | 42% | Pleasant | 86% |
| | "Those slimy | Motherly | 38% | | | |
| | | Sex | 21% | 29% | Pleasant | 68% |
| 478 | things" | Joy | 21% | | | |
| 313 | "Clever" | Joy | 74% | 45% | Pleasant | 66% |
| | | | | | Unpleasant | 25% |
| 451 | None | Sorrow | 21% | 38% | Pleasant | 65% |
| 178 | Surprise | Joy | 50% | | | |
| | | Surprise | 21% | 42% | Pleasant | 50% |
| *Situation 15—Decapitation of a Rat* | | | | | | |
| | | | | | Unpleasant | 49% |
| 257 | None | Joy | 47% | 59% | Pleasant | 46% |
| | | | | | Pleasant | 22% |
| 254 | None | Sorrow | 27% | 33% | Unpleasant | 50% |
| *Situation 16—Mental Multiplication with Electrical Stimulation* | | | | | | |
| | | | | | Pleasant | 65% |
| 58 | Afraid | Joy | 59% | 26% | Unpleasant | 25% |
| 608 | Befuddled | Maternal | 52% | | | |
| | | Joy | 42% | 37% | Pleasant | 63% |
| 116 | Pain | Sorrow | 83% | 38% | Unpleasant | 86% |

| 1 | 2 | 3 | | 4 | 5 | |
|---|---|---|---|---|---|---|
| 585 | Anger and pain | Sorrow<br>Joy | 21%<br>49% | 20% | Unpleasant<br>Pleasant | 21%<br>21% |
| *Situation 18—Relief* | | | | | | |
| 59 | Didn't believe it was the end | Joy | 97% | 60% | Pleasant | 82% |
| 461 | Said we were cruel | Sex<br>Joy<br>Maternal | 20%<br>48%<br>20% | 27% | Unpleasant | 25% |
| 747 | Surprised and glad | Anger<br>Fear | 35%<br>29% | 48% | Unpleasant | 75% |

Careful study of this table warrants the following statements:

(1) It is practically impossible to name correctly a photograph taken in an emotional situation, either as to the emotion's being experienced by the subject or the situation's giving rise to the expression.

(2) The titles which are given to such photographs are mainly such general statements as Joy, Sorrow, Maternal, Surprise, or the like. These general statements are often partially contradictory.

(3) When the emotional labels are classified as to whether they "Agree," "Disagree," or have "No Relation" to the "Actual Introspection" we find that 31% agree, 47% disagree, and 22% bear no relation.

(4) When the emotional labels are classified as to whether or not, in the opinion of the writer, they are appropriate, inappropriate, or bear no relationship to the actual situation in which the picture was taken, we find 43% may be regarded as appropriate, 48% inappropriate, and 9% bear no relationship.

(5) The average percentage of certainty for these judgments was 39.

(6) When our observers were asked to characterize the situation which might give rise to the expression shown on the photograph, we find that they resort to four predominant generalities: Pleasant, Unpleasant, Religious, and Maternal. No other situation than these four was mentioned in more than 20% of the judgments.

(7) If one classifies the situation which the judges assigned as appropriate with respect to agreement, disagreement, or mutual contradiction (Pleasant and Unpleasant both assigned) on the part of the judges, one finds 38% agree, 38% disagree, and 24% are mutually contradictory.

In addition to the pictures taken in actual situations we presented the judges with pictures which had been taken when the subject was endeavoring to reproduce his behavior as he remembered it from having gone through the situations previously. The compilation of these figures is given below.

| 1 | 2 | 3 | 4 | | 5 | 6 |
|---|---|---|---|---|---|---|
| Picture number | No. of situation being reproduced | Actual introspection | Judgment and percentage frequency | | Per cent certainty | Situation noted which might give such expression |
| 2112 | 1A | Just laughed | Joy<br>Motherly | 38%<br>33% | 35% | Pleasant 85% |
| 2005 | 2A | Pleasant | Sorrow<br>Fear | 34%<br>20% | 50% | Unpleasant 60%<br>Religion 28% |
| 2114 | 2A | Pensive | Anger | 24% | 35% | Unpleasant 39% |
| 2081 | 4A | No feeling | Anger<br>Attention | 20%<br>22% | 48% | Unpleasant 62% |
| 2013 | 5A | Unpleasant | Disgust | 48% | 29% | Unpleasant 74% |
| 2051 | 5A | Amused | Joy<br>Motherly | 36%<br>52% | 32% | Pleasant 35%<br>Maternal 35% |
| 2126 | 9A | Disgust | Sorrow | 41% | 35% | Unpleasant 45% |
| 2057 | 10A | Feeling of strangeness | Motherly | 23% | 38% | Maternal 35% |
| 2020 | 10A | No emotion | Sorrow<br>Devout | 43%<br>24% | 40% | Unpleasant 60% |
| 2026 | 13A | Effort | Sorrow<br>Sex | 24%<br>21% | 34% | Unpleasant 52% |
| 2134 | 13A | None | Sorrow | 59% | 35% | Unpleasant 62% |
| 2028 | 14A | Slight fear and repulsion | Sorrow | 33% | 29% | Pleasant 45%<br>Unpleasant 25% |
| 2102 | 14A | Unpleasant | Anger<br>Attention | 42%<br>34% | 41% | Unpleasant 58%<br>Religion 21% |
| 2135 | 14A | None | Sex<br>Maternal | 32%<br>23% | 35% | Pleasant 27% |

| 1 Picture number | 2 No. of situation being reproduced | 3 Actual introspection | 4 Judgment and percentage frequency | | 5 Per cent certainty | 6 Situation noted which might give such expression |
|---|---|---|---|---|---|---|
| 2066 | 14A | "Some kick" | Joy<br>Maternal | 47%<br>44% | 29% | Pleasant 32%<br>Maternal 41% |
| 2065 | 14A | "Some kick" | Joy | 72% | 32% | Pleasant 62% |
| 2067 | 15A | "Can't imagine it" | Joy<br>Maternal | 68%<br>26% | 43% | Pleasant 88% |
| 2140 | 16A | Irritated | Joy<br>Maternal<br>Embarrassed | 27%<br>30%<br>27% | 27% | Embarrassed 32% |
| 2143 | 18A | Felt good | Sex | 36% | 32% | Unpleasant 36% |
| 2071 | 18A | Relief | Joy | 100% | 48% | Pleasant 81% |

The figures above seem to justify the following statements:

(1) Observers are unable to name correctly either the emotion posed for or the situation which the subject was trying to reproduce, when these observers have only a photograph of the face on which to base their judgment.

(2) When the relationships between "Actual Introspection" and "Judgment" are classified as agreement, disagreement, or no relationship, we find 28, 41, and 31 per cent, respectively.

(3) When the relationships between "Actual Introspection" and "Situation noted which might give such expression," are classified as agreement, disagreement, no relationship, or mutual contradiction (Pleasant and Unpleasant), we get 38, 35, 23, and 4 per cent, respectively.

(4) The average percentage of certainty for these judgments was 36.

## Discussion

The most outstanding fact brought out in this study is that observers cannot make a statement more accurate than one would expect by chance concerning the emotion which a subject was undergoing when photographed or concerning the probable situation which might give rise to such an expression. The percentages of agreement which have been noted above are as high as they are because of the

generality of the judgment. A statement that the photograph indicates "Joy" or a "Maternal Expression" covers an exceedingly wide range of possibilities. The description of the situation which might give rise to the expression, in such terms as "Pleasant" or "Unpleasant," is likewise exceedingly general, so bringing up the percentages of agreement without adding anything specific to the judgment.

The point of mutual contradiction is interesting, eight pictures being classified both as appropriate for a pleasant or an unpleasant situation in more than 20 per cent of the judgments.

How are the findings of this experiment to be reconciled with those of Langfeld (1918), Ruckmick (1921), Gates (1925), Buzby (1924), and others who have submitted series of pictures to observers and found a certain degree of ability on the part of the judges in naming the emotion portrayed? The explanation would seem to lie in the fact that the pictures which these other experimenters have used are not true portraits of the facies of emotion but are rather pictures of the socialized and to a large extent conventionalized reactions which are used as supplementary language mechanisms. There is little or no evidence that such expressions occur in "emotion" except by chance (Landis, 1924). Gates's (1925) study shows that this ability to interpret the conventional expression increases in acuity during childhood; a finding not to be reconciled with Darwin's doctrine of an instinctive ability.

Sherman's (1927) work, together with the present paper, indicates that the human observer is at a loss when confronted with "native" behavior and with only that behavior. That is to say, practically all of our interpretations of the reactions of our fellow-beings are based upon *both* the reactions (expressions) and the situation surrounding the behavior. A description of behavior without mention of the surroundings or situation would be practically meaningless. Some further evidence to this is shown in the results of that portion of this experiment in which we asked the observers to name the emotion portrayed when given photographs taken while the subject was trying to reproduce the behavior or "feelings" which he had had in the original situation. These pictures of facial expression—of behavior—show some conventional expressions but not to any marked degree. These our judges were no more accurate in naming than those taken when the subject was pretty largely off his guard due to emotional excitement.

How can the findings be reconciled with Darwin's statement that the recognition of a smile, a frown, etc., are instinctive in the human infant? It seems extremely doubtful that Darwin's evidence is well based in fact. Any stimulation which results in an increased tonicity of the body is very apt to exhibit itself in a facial pattern which we

call a smile. Dumas (1923) has shown that the smile is the facial pattern of hypertonus. Hence we might assume that the visual and auditory stimulation furnished by the nurse would be apt to evoke a smile in the infant in a truly reflex manner. The additional stimulation following the smile might conceivably result in crying. The establishment of conditioned responses at an early age has been demonstrated by Watson and Rayner (1920). The point of emotional facilitation of the conditioned response has been shown by Pavlov (1927). The behavior of the infant which Darwin considered as instinctive we would explain in terms of the reflex and conditioned response.

Finally, what is the relation of these findings to our system of knowledge concerning emotion? Several years ago (Landis, 1924) I advanced the hypothesis that any description of an emotion was incomplete when it dealt only with the reactions of the organism itself, that an emotion was an emotion not because of any particular pattern of bodily reactions or temporal relationships but because of certain relationships existing between the reacting organism and its environment. This experiment offers further evidence to that hypothesis.

## Summary

Photographs taken during a series of emotional situations in which there was little question of the emotional excitement of the subject (Landis, 1924) were shown to a group of observers, who were asked to say what "emotion" the picture depicted, in what situation the expression might be obtained and the percentage of certainty of their judgment. The following facts were brought out.

(1) The names given by the observers to the expressions were no more often correct than one would expect by chance. It is practically impossible to name accurately the "emotion" being experienced by a subject when one has only a photograph of the face on which to base the judgment.

(2) The titles given the pictures more often disagree than agree with the verbal report which the subject had given just after the picture had been taken.

(3) When the judgments are classified as to whether or not they seem appropriate names for reactions to the actual situations in which the photographs were taken, we find that 43 per cent may be regarded as appropriate and 48 per cent as inappropriate.

(4) Judgments of photographs taken when the subject was trying to reproduce his "feelings" or behavior which had taken place in the original situation were only very slightly more accurate than judgments of photographs taken in the actual situations.

(5) The descriptions of the situation which might give rise to the expression were very general. The four headings, Pleasant, Unpleasant, Maternal, and Religious, cover practically all descriptions occurring in more than 20 per cent of the cases. For eight of the photographs the mutually contradictory descriptions, Pleasant and Unpleasant, each occur in more than 20 per cent of the judgments.

(6) The percentage of certainty felt by the judges was 39 for the pictures taken during actual emotional excitement and 36 for pictures taken when that excitement was being recalled.

(7) A consideration of these findings with regard to other similar experiments and to the problem of emotion is given.

## Bibliography

Buzby, D. E. The interpretation of facial expression. *Amer. J. Psychol.*, 1924, **35**, 602-604.

Darwin, C. The expression of the emotions in man and animals. New York: Appleton, 1873.

Dumas, G. Traité de psychologie, I. Paris: Alcan, 1923. See pp. 631-632.

Gates, G. S. A test for ability to interpret facial expressions. *Psychol. Bull.*, 1925, **22**, 120.

Landis, C. Studies of emotional reactions: II. General behavior and facial expression. *J. Comp. Psychol.*, 1924, **4**, 447-509.

Langfeld, H. S. The judgment of emotions from facial expressions. *J. Abnorm. Psychol.*, 1918, **13**, 172-184.

Pavlov, I. P. Conditioned reflexes. London: Oxford Univ. Press, 1927. See pp. 395-411.

Ruckmick, C. A. A preliminary study of the emotions. *Psychol. Monog.*, 1921, **30**, (no. 136).

Sherman, M. The differentiation of emotional responses in infants: I. Judgments of emotional responses from motion picture views and from actual observation. *J. Comp. Psychol.*, 1927, **7**, 265-284.

Watson, J. B., & Rayner, R. Conditioned emotional reactions. *J. Exper. Psychol.*, 1920, **3**, 1-14.

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## L'INTERPRÉTATION DE L'EXPRESSION FACIALE DE L'EMOTION

(Résumé)

Des photographies faites pendant une série de situations émotionnelles où il n'y avait aucun doute de l'excitation émotionnelle du sujet, ont été montrées à un groupe d'observateurs à qui on avait demandé de dire quelle était "l'émotion" montrée dans la photographie; dans quelle situation on pourrait obtenir l'expression et le pourcentage de certitude de leur jugement. On a établi les faits suivants:

1) Les noms donnés aux expressions par les observateurs n'ont pas été corrects plus souvent qu'on compterait trouver au cas du hasard.
2) Les titres donnés aux photographies ne sont pas, pour la plupart, en accord avec le rapport verbal donné par le sujet immédiatement après la prise de la photographie.
3) Quand on classifie les jugements, considérant s'ils sont des noms justes pour les réactions aux situations mêmes où l'on a fait les photographies on trouve qu'on peut considérer 43% justes et 48% peu justes.
4) Les jugements des photographies faites quand le sujet essayait de reproduire ses "émotions" ou comportement ayant eu lieu dans la situation même n'ont été qu'un peu plus justes que les jugements des photographies faites dans les situations mêmes.
5) Les descriptions de la situation qui aurait pu causer l'expression ont été très générales.
6) Le pourcentage de certitude éprouvée par les juges a été de 39 pour les photographies faites pendant la vraie excitation émotionnelle et de 36 pour les photographies faites au moment du rappel de cette excitation.

LANDIS

## ÜBER DIE DEUTUNG DES GESICHTSAUSDRUCKS BEI GEMÜTSBEWEGUNGEN

(Referat)

Photographien die man während einer Reihe von aufregenden Situationen aufgenommen hatte, wobei die Gemütsbewegung der betreffenden Personen ausser Frage stand, zeigte man einer Gruppe von Vpn, die entscheiden sollten, welche Art der Erregung das Bild darstellte; welche Situation wohl den Gesichtsausdruck habe verursachen können; und ebenfalls den Prozentsatz der Gewissheit ihres Urteils. Die Ergebnisse dieser Untersuchung sind wie folgt:
1) Die Namen welche Vpn den verschiedenen Gesichtsausdrücken beilegten, trafen nicht öfter das Richtige als man es auch vom Zufall erwarten könnte.
2) Die den Bildern gegebnen Titel stimmten meistens nicht mit dem überein was die Vp gleich nach der Aufnahme der Photographie berichtete.
3) Nach systematischer Prüfung der Urteile, erweist es sich, dass nur 43 der Namen passende Bezeichnungen für die Reaktionen der wirklichen Situationen bei welchen die Aufnahmen stattfanden, waren, dagegen 48 Prozent als nicht passend bezeichnet werden mussten.
4) Beurteilungen der Photographien die aufgenommen wurden, wenn die Vp versuchte die betreffenden Gefühle oder das Verhalten zu reproduzieren, waren nur wenig zutreffender als die Beurteilungen der Photographien, welche unter den wirklichen Umständen genommen wurden.
5) Die Beschreibungen der Situationen, welche die Gesichtausdrücke verursachen könnten, waren sehr allgemein gehalten.
6) Der Prozentsatz der Gewissheit, welche die Richter fühlten, war 39 für die Photographien, welche während der wirklichen Gefühlserregung aufgenommen wurden, und 36 für die Photographien der reproduzierten Erregung.

LANDIS

## ХАРАКТЕРИСТИКА ВЫРАЖЕНИЯ ЛИЦА ПРИ ЭМОЦИЯХ.

(Реферат).

Группе испытуемых были предъявлены фотографии, заснятые во время эмоционального состояния лица, аффективное возбуждение которого не вызывало сомнений. Испытуемым дана была инструкция указать, какие эмоции фотографии выражали, при каких условиях эти выражения могли быть получены и в каком проценте своих суждений они были уверены.

Опыт этот дал следующие результаты:

1. Характеристика, которую испытуемые давали тому или иному выражению, была не более правильной, чем можно было ожидать от случайных ответов.

2. Характеристика, которую испытуемые давали фотографиям, не совпадала со словесным отчетом, которое давало снимавшееся лицо сейчас же после съемки.

3. При анализе суждений об эмоциональном состоянии заснятых лиц было установлено, что 43% всех суждений были правильными и 48% неправильными.

4. Суждения о снимках, когда снимавшееся лицо пыталось воспроизвести свои эмоции или поведение, которое имело место в первоначальной ситуации, были только немного более точными, чем суждения о фотографиях при реальном аффекте.

5. Описание ситуации, при котором могло возникнуть данное выражение, было очень обще.

6. Процент уверенных суждений был 39 для снимков, сделанных при реальном эмоционального возбуждения и 36 при репродуцированном возбуждении.

**Лэндис (Landis).**

# THE EFFECT OF EXHAUST GAS ON THE PERFORMANCE IN CERTAIN PSYCHOLOGICAL TESTS*†

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The increase in the number of automobiles and trucks and the accompanying increase in the exhaust gas caused thereby has given rise to some new physiological and psychological problems. These problems involve both the immediate and the cumulative effects upon the human organism. In order to ascertain such effects, the experimentation must deal with a problem such as the case of a driver in traffic who has been temporarily exposed to a high concentration of exhaust gas and who must make the correct response or endanger both his own life and the lives of others. Another problem is that of a traffic policeman or a mechanic working in a garage day after day and exposed to varying degrees of carbon monoxide gas (CO). In this latter instance it is not the immediate response that is of such great importance; it is rather a question of maintaining output or the correct response for a period of time. These conditions in themselves are rather difficult to duplicate in a laboratory, but experiments have been devised that should throw some light on the effects produced by exhaust gas. To what extent identical factors may be present in the psychological tests used in this experiment and in the actual working conditions just outlined cannot be determined. These tests will be considered as standardized performances involving certain psychological processes of attention, muscular coordination, memory, etc., capable of objective measurement and no attempt will be made to establish the relation between the results obtained from the psychological tests and a practical situation.

A second set of interests, related in an interesting manner to those which have been mentioned, operated to make a joint investigation

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possible. At the Mellon Institute, the members of the Simmons Fellowship for the Study of Sleep were interested in the development of performances capable of testing impairment due to exertion, and the recovery which followed sleep. Their working hypothesis, expounded by Verworn, is that fatigue and narcosis are probably special forms of cell-asphyxiation, and are therefore essentially similar except as to their history. They reasoned, therefore, that a suitable "test" of narcosis or of asphyxiation should also be a suitable test of some degree of fatigue and vice versa. The results of the present experiment thus promised a basis of tentative acceptance or rejection of test-performances to be utilized in their future studies. A combined physiological and psychological experiment was therefore instituted by the United States Bureau of Mines[2] at its Pittsburgh Experiment Station. Only the psychological results will be discussed in this report because the physiological tests were under the direction of another investigator.

Six medical students from the University of Pittsburgh were exposed daily to exhaust gas for a period ranging from about 3½ to 6 hours, with the exception of certain control days. The time of exposure depended upon certain physiological conditions, such as headaches, pulse-rate, CO content of the blood, and nausea. It might be mentioned here that the danger involved in a test of this nature made it somewhat difficult to get subjects who would be willing to submit to prolonged experimentaion. Two other factors mitigated against ideal working conditions for psychological experimentation: the cost of running a control group under similar conditions made that procedure prohibitive, and the fact that the physiological experiments were directed primarily towards the cumulative effects prevented the insertion of control days at times when they would have been most desirable.

Exhaust gas from a gasoline engine was forced into the chamber in which the men stayed and worked. The chamber was so arranged that the air was changed every 8 minutes, thus preventing depletion of oxygen. An effort was made to control the temperature, although on several very hot days it did rise above the comfort zone. The amount of CO in the chamber air was continuously recorded by

[2]Appropriations from the New York State Bridge and Tunnel Commission and the New Jersey Interstate Bridge and Tunnel Commission and the Simmons Foundation of the Mellon Institute, University of Pittsburgh, made it possible to carry on the psychological part of this research. The writers acknowledge their indebtedness to Dr. H. M. Johnson, of the Mellon Institute, and William P. Yant, supervising chemist, Health Laboratory Section of the Bureau of Mines, for their helpful suggestions and timely aid during the experiment.

means of a carbon monoxide recorder,[3] which was accurate to about 0.2 p.p. 10,000 air.[4]

## Outline of Tests

The psychological tests employed in the investigation were steadiness, tapping, dynamometer, substitution, arithmetic, location memory, and the so-called Felix test. Not all of these tests were employed throughout the experiment, and certain modifications in procedure were introduced in some of the tests during the experiments. These modifications will be described later.

## Apparatus and Methods

In the steadiness test, a Whipple type of plate and stylus[5] was used. The plate consists of a series of circular holes of different diameters in a thin strip of brass. The size of the holes employed in this experiment ranged in diameter from 11/64 to 7/64 inch. The plate and stylus were electrically connected so that a graphic record of the contracts was secured. A record of the performance with the right hand was obtained for each reactor while seated for a 20-second period in each hole. No lapse of time was allowed between the performance on successive holes which were tried in the order from larger to smaller. The task is one involving hand and eye coordination in which the reactors must prevent the stylus from touching the sides of the holes. The score is the total number of contacts made in all five holes.

In the tapping test a Johns Hopkins double tapping plate[6] was used. The subjects, holding a stylus in the right hand, were instructed to begin tapping on the plate at the word "go" and to continue tapping as rapidly as possible until told to stop. This plate was connected with a magnetic marker writing on a kymograph and at the same time a chronographic record in seconds was taken. The score for this test is the total number of taps made in a 2-minute period.

In the dynamometer test, a Smedley dynamometer[7] which gives

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[3]Katz, S. H., Reynolds, D. A., Frevert, H. F., & Bloomfield, J. J. A carbon monoxide recorder and alarm. Bureau of Mines Technical Paper, No. 355, 1926. Pp. 35.

[4]For a complete description of the room in which the work was conducted, see: Sayers, R. R., Meriwether, F. V., & Yant, W. P. Physiological effects of exposure to low concentrations of carbon monoxide. *Pub. Health Stud.*, 1922, reprint no. 748, 1127–42.

[5]Whipple, G. M. Manual of mental and physical tests: Simpler processes. Baltimore: Warwick & York, 1914. See p. 157.

[6]Dunlap, K. Improved form of steadiness tester and tapping plate. *J. Exper. Psychol.*, 1921, **4**, 430-433.

[7]Whipple, G. M., *loc. cit.*, 101.

a record of the muscular strength of handgrip in terms of kilograms was used. The subjects were required to make 10 successive pulls, exerting their maximum strength on each pull. The subjects were told what they made on each trial as each record was recorded.

The substitution test,[8] a modified form of the one used by Johnson and Paschal, was conducted as follows. By means of optical projection there was exposed to the whole group of subjects the following material: (*a*) the letters of the English alphabet, arranged in a single line in their regular order; (*b*) immediately below (*a*), another line, containing the same letters in another order, determined by a fixed system of rules; this line was called the "code"; (*c*) below the alphabet and code were 300 consonants, arranged in three squares of 100 letters each, constituting the material for which substitutions were to be made. Different sets of material and different codes were used at different presentations but all were of very nearly equal difficulty. Each set was exposed for five minutes. The subject's task was to encode as many letters within that time as he could, beginning with the first and proceeding in a definite order. The score was determined by the number of letters correctly encoded within the time allowed. At each work period two slides were presented. A 5-minute work period was given for each slide, with a 1-minute rest period between the slides.

In the arithmetic test, continuous adding was used. It involves a rather complicated series of associations, since the reactors were required to make accurate mental additions and remember the order in which the numbers were to be added. At each work period the reactors were given a different two-place number to which they were to add 38, to the total thus obtained 47 was to be added, to that total 29, and to that total 56. To the total thus obtained, this series was to be repeated in the same manner. This was to continue until the command "stop" was given. The score assigned in this is the total number of correct additions made in a 6-minute work period.

In the location memory test,[9] which is somewhat similar to the spot pattern test, a form devised at the Johns Hopkins University was used. It consisted in having the subjects make the same pattern on a sheet by filling in certain circles shown just previously on a slide. For this purpose 4 sets of 6 slides were made. Each set contained 6 different patterns, one each of 3, 4, 5, 6, 7, and 8 spots. As each set could be placed in the projector in 4 different positions,

[8] Johnson, H. M., & Paschal, F. C. Psychological effects of deprivation of oxygen: Deterioration of performance as indicated by a new substitution test. *Psychobiol.*, 1920, **2**, 193–236.

[9] Johnson, B. J. Mental growth of children. New York: Dutton, 1925. See pp. 129-130.

the equivalent of 16 different sets was available. The slides were presented in the order just given, and only one exposure of 5 seconds was allowed for each slide. The score is the number of circles correctly filled minus the number of circles incorrectly filled.

In the Felix Test[10] the apparatus described by Dunlap was employed. It is essentially a form of continuous reaction apparatus. The subject is required to press one of four keys corresponding to a particular light momentarily flashed by a make-and-break device, which is arranged so that the same series is never repeated. The lights appeared at the rate of 70 a minute. By means of a series of magnetic markers and a kymograph, a record is obtained of all errors, premature responses, and late responses. Each test period lasted 15 minutes. In this task the attention must be sustained to a high degree for the duration of the work period, as the reactors are unable to predict when any one of the lights will be flashed. The score is determined by the number of late responses made in each 15-minute work period.

## Discussion of Results

In the discussion of results it must be remembered that all of these tests were not given every day because the time required and the influence of a test upon those following prohibited that procedure. The results will therefore be treated as successive work periods, disregarding the time intervals. An attempt was made, however, to equalize this condition. The morning work was given as soon as the subjects entered the chamber and before they were subjected to gas. The usual time for working was from 9 to 10 o'clock. The afternoon work was given just before the men left the chamber for the day.

The effects produced by the gas must be considered in two different ways: first, one must consider whether the subjects make a better or worse performance after having been subjected to gas; and secondly, one must consider whether during any given work period the subjects tire more rapidly when they have been exposed to gas than they do when they have not been subjected to gas. This point will be considered with respect to the tapping, dynamometer, and Felix tests.

No distinction is made in the curves for the quantitative differences in the amount of gas administered to the subjects. This was done for two reasons. As no difference could be found between the performance after exposure to any of the concentrations of gas and

[10]Dunlap, K. Apparatus and methods for measurement of psychological condition. *J. Comp. Psychol.*, 1926, **6**, 133-138.

the performances on control periods, it did not seem feasible to attempt to separate the gas periods on a quantitative basis. Another factor operated against any difference in performance that might be caused by the different concentrations employed. All of the subjects were brought to about the same physiological point or degree of poisoning with CO, so to speak, for all the different gas conditions. That is to say, they were examined after the blood had absorbed about the same amount of CO, although the time required to bring about the saturation markedly increased with decreasing amounts of CO in the air.

### *Felix Test*

Figure 1 shows the number of late responses for each working period for each subject on the Felix test. The premature and incorrect responses were so few that they were disregarded. Each subject was given a 10-minute work period on 7 successive days before the actual experiment commenced. This was done in order to reduce the practice effect. If the gas produced any deleterious effect, the solid circles, which represent the afternoon work periods on the curve, should occupy the high points. If these are compared with the partly filled circles (morning performances) and the open circles (control day afternoon performances), it will be found that there is no general tendency for the solid circles to occupy the peaks and for the other circles to occupy the low points. This condition holds true for all 6 subjects. Tests were made also from 10 P.M. to 12 P.M., or 6 to 8 hours after exposure to gas, in order to determine whether the gas effect might be a delayed one. The results of these performances are represented by circumscribed cross and circumscribed plus on the curves. The former signs represent the scores after the subjects had been exposed to gas during the day; the latter signs represent the scores made on control nights. These have about the same relation as do the solid and unfilled circles—that is to say, the effects produced by the gas are probably no greater than the variation from work period to work period.

### *Dynamometer Test*

Figure 2 shows the total scores for each work period made by each subject on the dynamometer test. This test was introduced after complaints of muscular weakness by subjects after exposure to gas. As this test was not started until late in the investigation, there are less work periods than for some of the other tests. On the dynamometer test as with the Felix test, the gas periods do not show any tendency to occupy either the high or low points on the curves. One point of general interest, however, is shown by the figures: contrary to ex-

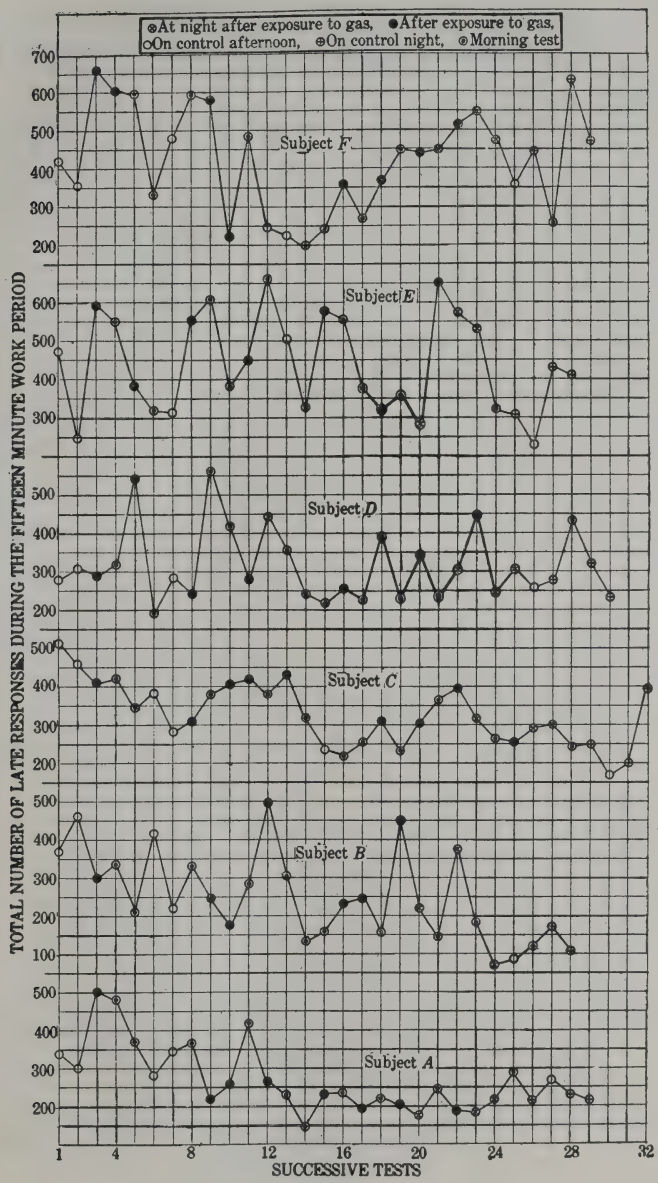


FIGURE 1

GRAPHS SHOWING THE PERFORMANCE OF DIFFERENT SUBJECTS IN THE FELIX TEST

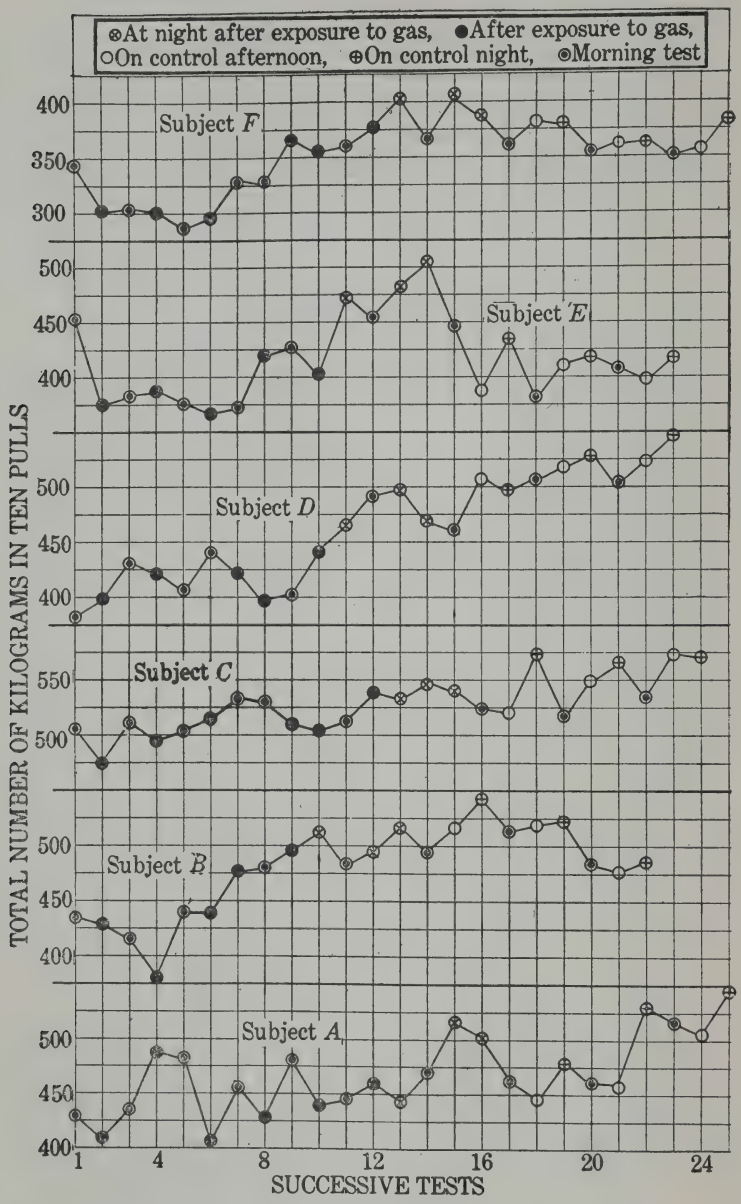


FIGURE 2

GRAPHS SHOWING THE PERFORMANCE OF DIFFERENT SUBJECTS IN THE DYNAMOMETER TEST

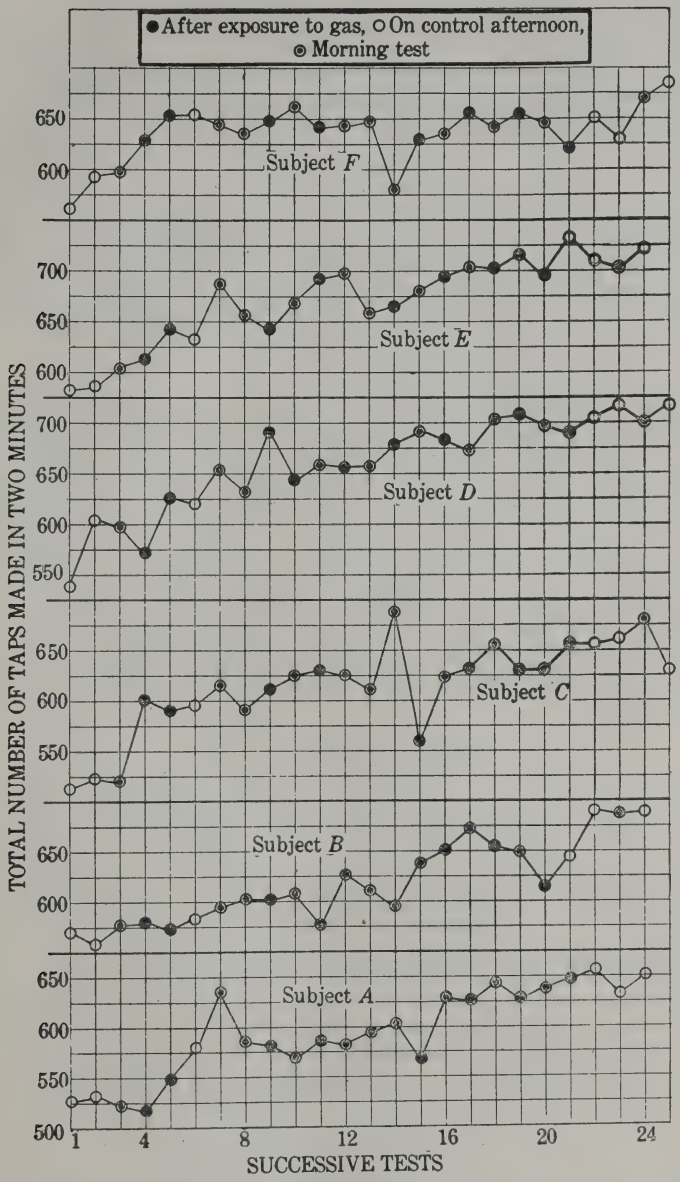


FIGURE 3

GRAPHS SHOWING THE PERFORMANCE OF DIFFERENT SUBJECTS IN THE TAPPING TEST

pectation, there is an increase in the muscular strength which takes place during a rather short practice period. The crosses and plus signs represent the performances made at night after gas and control days respectively. These night performances do not show any conditions that are different from the day performances.

### *Tapping Test*

Figure 3 shows the total number of taps made by each subject for each work period. As the tapping test was not administered at night, no crosses and plus signs appear on the curves. All of the subjects show improvement in tapping, but there is no decided difference between the gas performance and the control performances.

### *Steadiness Test*

Figure 4 shows the scores made on the steadiness test. Although each hole that was used has been studied separately, curves representing the scores made on each hole will not be included. The curves here represent the total number of contacts made in all five holes for the total time required in a single work period. These curves do show a slight tendency for the filled circles to occupy the high peaks, a fact which seems to hold true for all 6 subjects. Some of the high peaks are, however, occupied by the partly filled and open circles. It is also true that some of the lowest points are occupied by the solid circles. The subjects do show a greater tendency to make a poorer score on this test after being exposed to gas than on any of the other tests.

### *Location Memory Test*

Figure 5 shows the scores made by each of the subjects on the location memory test. This score was obtained by subtracting the number of misplaced spots from the number of spots correctly placed. Although this may not be the most desirable way of scoring for this test, it serves the purpose adequately because we are interested only in comparing different work conditions. As a low score on this test indicates a poor performance, it might be expected that the solid circles would occupy the low points on the curves. But this does not happen to be the case. The location memory test, like the tests mentioned before, does not show anything conclusive concerning the effects of exhaust gas in so far as a comparison of the total scores under different work conditions is concerned.

### *Arithmetic Test*

Figure 6 shows the performance of the different subjects in the arithmetic test. This test shows that there is very little effect pro-

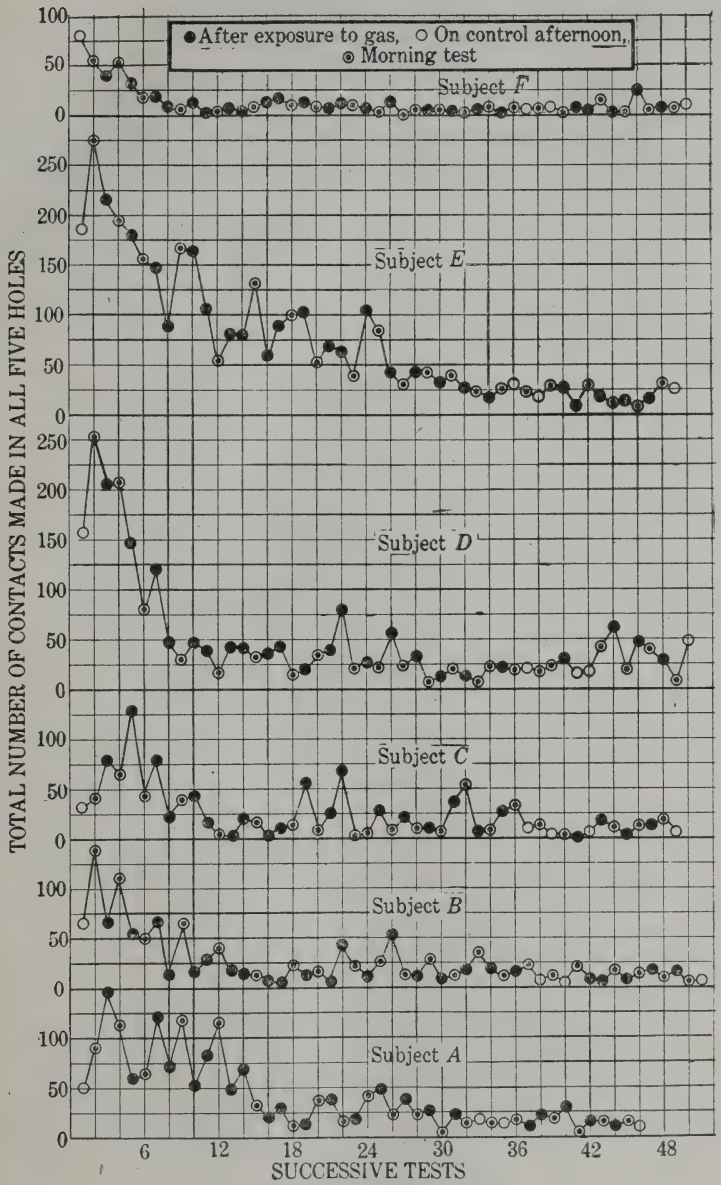


FIGURE 4

GRAPHS SHOWING THE PERFORMANCE OF DIFFERENT SUBJECTS IN THE STEADINESS TEST

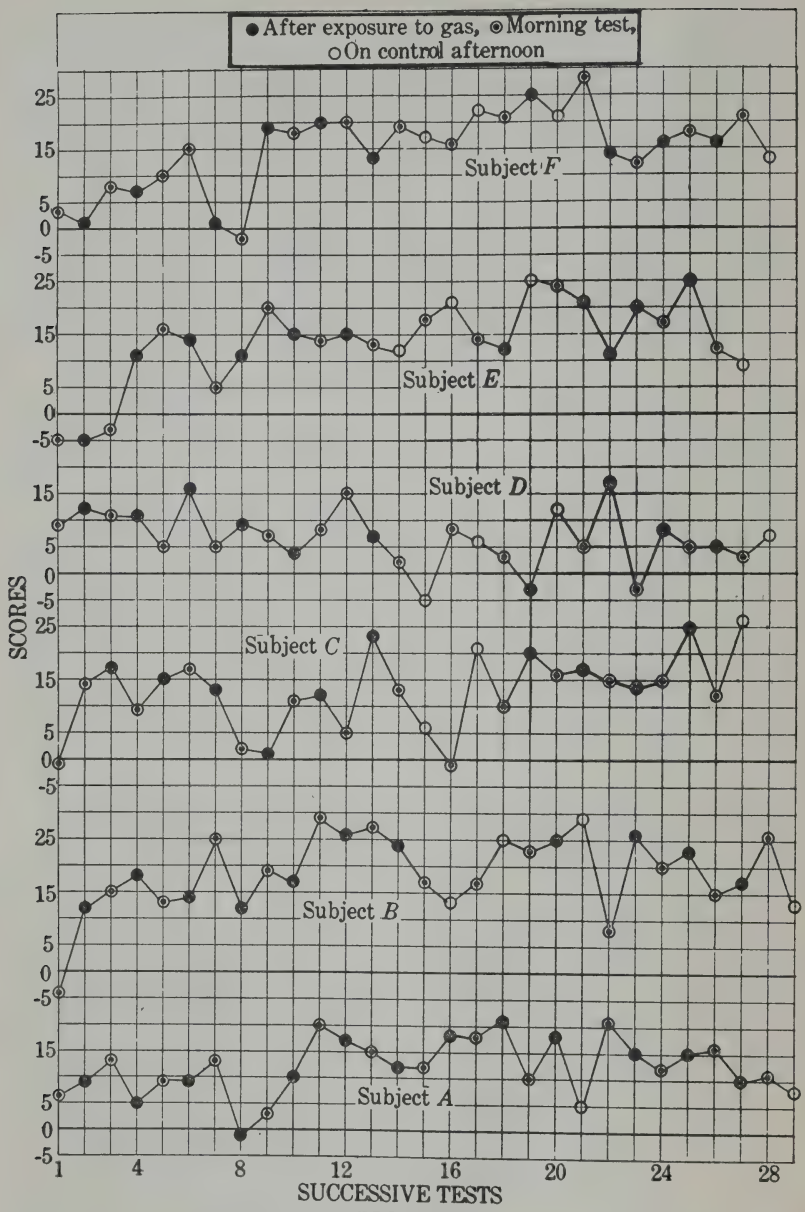


FIGURE 5

GRAPHS SHOWING THE PERFORMANCE OF DIFFERENT SUBJECTS IN THE LOCATION MEMORY TEST

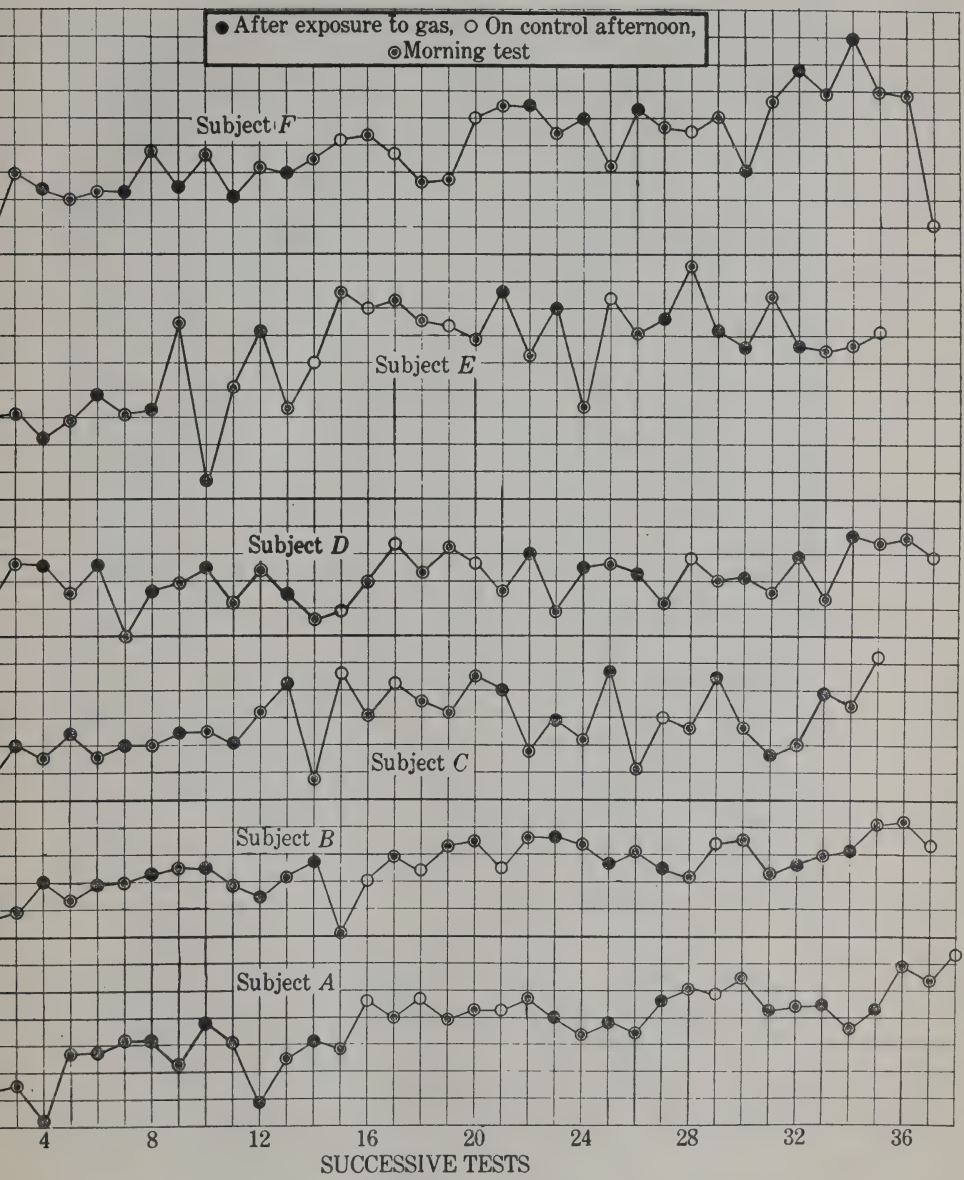


FIGURE 6

GRAPHS SHOWING THE PERFORMANCE OF DIFFERENT SUBJECTS IN THE ARITHMETIC TEST

duced by the gas. The scores in this test are obtained by taking the total number of correct additions for the 6-minute work periods. The solid circles occupy a fair number of high points in all six of the curves. The partly filled circles and the open circles (morning and control afternoon performances respectively) are so distributed that it appears as though the only factor is that of daily variation.

Figures 7 and 8 show the results obtained from the substitution tests. These curves show a two-fold relation: the relation between successive performances is shown by the different type of symbols used; relation between the two 5-minute work periods at any given sitting is shown by the two symbols of the same kind appearing close together. For example, the curve representing the performance of subject A has for its first two points two partly filled circles. These show the scores made in the morning on the first and second 5-minute periods of work. The next two points are marked by open circles (control afternoon periods). If the relation between the first and second symbol of the same kind is studied, one can determine whether there is a tendency to increase or decrease the score under the different working conditions. Careful scrutiny of each curve does not show any general tendency toward any increase or decrease in scores on the second part of the work period after the individual has been subjected to gas. This statement holds true also for the morning and afternoon control performances. If the high and low points of the curves are considered, it will be found again that a great variation exists under the different working conditions. There is a haphazard distribution of the points representing the gas periods with reference to the positions which they occupy on the curves. The chance of making a poor score under the control conditions is about equal with that under gas conditions. The night performances show no more consistency than do the day performances.

Tables 1, 2, 3, and 4 show the performance made at the beginning and at the end of the work periods in the dynamometer, tapping, Felix, and substitution tests.

*First and Last Five Pulls in Dynamometer Test*

Table 1 shows the averages made under the different conditions by each man for the first and last 5 pulls in the dynamometer test. Although the average made by one man after being exposed to 3 parts of CO per 10,000 of air is not directly comparable (because of the learning effect) to the average made by the same man on control days, the ratio between the averages of the first 5 pulls and the last 5 pulls after being exposed to gas is comparable to the ratio between the averages of the first and last 5 pulls on control days. Thus, if the ratios obtained from the various working conditions are examined,

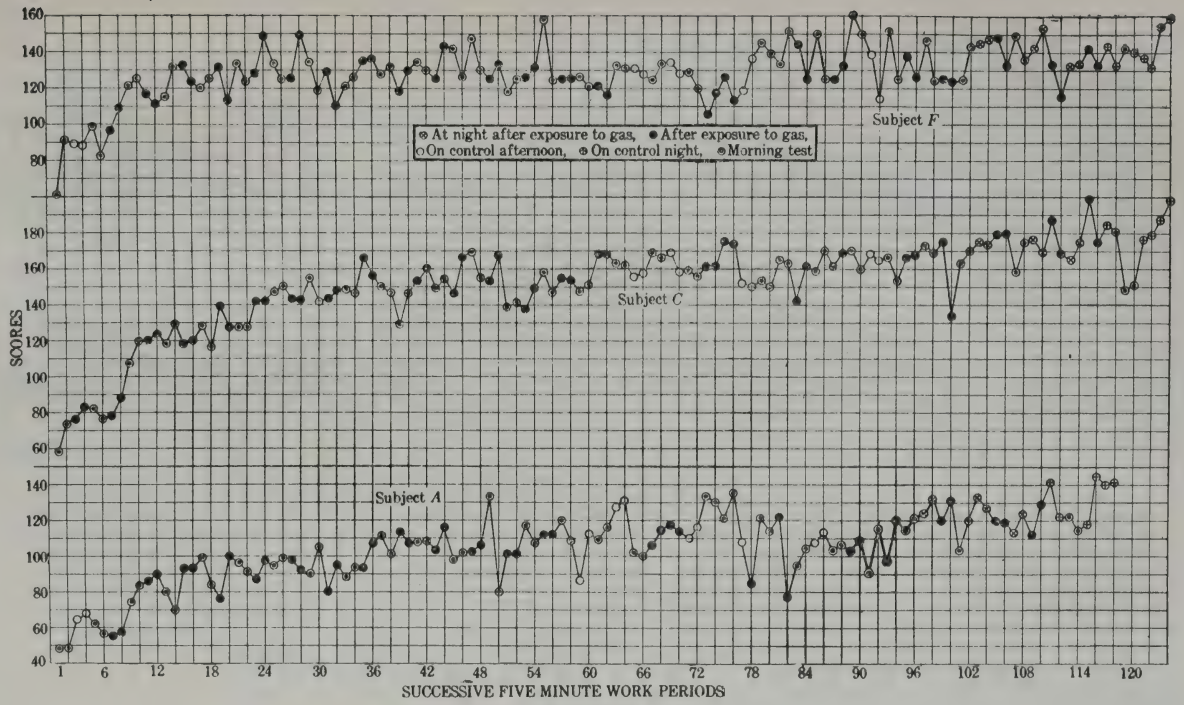
⊗ At night after exposure to gas, ● After exposure to gas,
○ On control afternoon, ⊙ On control night, ⊛ Morning test
Subject F
Subject C
Subject A
SCORES
SUCCESSIVE FIVE MINUTE WORK PERIODS

FIGURE 7

GRAPHS SHOWING THE PERFORMANCE OF DIFFERENT SUBJECTS IN THE SUBSTITUTION TEST

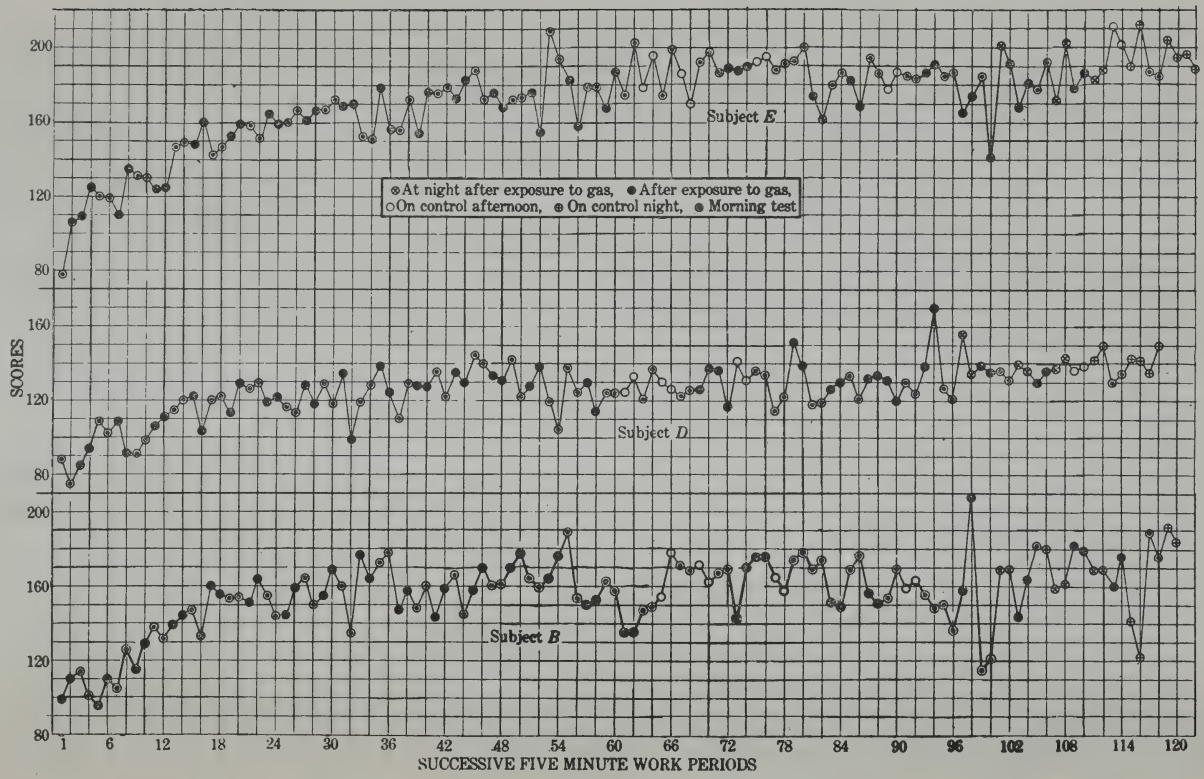
⊗At night after exposure to gas, ●After exposure to gas,
○On control afternoon, ⊕On control night, ◉Morning test
Subject E
Subject D
Subject B
SCORES
200
160
120
80
1 6 12 18 24 30 36 42 48 54 60 66 72 78 84 90 96 102 108 114 120
SUCCESSIVE FIVE MINUTE WORK PERIODS

FIGURE 8

GRAPHS SHOWING THE PERFORMANCE OF DIFFERENT SUBJECTS IN THE SUBSTITUTION TEST

it will be found in the case of subject A that there is very little difference in his muscular strength from the beginning to the end of the work period. The ratios between the average of the first and last 5 pulls are 1.08 (A.M., 3 parts CO), 1.03 (P.M., 3 parts CO), 1.05 (A.M. control), 1.04 (P.M. control), .96 (nights, 3 parts CO), and 1.01 (night control). A greater ratio means a greater dropping off in performance. If muscular weakness is produced by exposure to CO, the higher ratios should be found in the afternoon and night performances after CO has been administered. There is no tendency for the higher ratios to be found under those conditions.

TABLE 1

PERFORMANCE IN DYNAMOMETER TEST

| | | *Averages for first and last 5 pulls* | | | | | |
|---|---|---|---|---|---|---|---|
| Subject | CO parts/10,000 | A.M. First | A.M. Last | Ratio | P.M. First | P.M. Last | Ratio |
| A | Control | 245.6 | 233.6 | 1.05 | 240.3 | 231.3 | 1.04 |
| | 3 | 235.7 | 217.4 | 1.08 | 226.3 | 220.3 | 1.03 |
| B | Control | 259.3 | 239.6 | 1.08 | 262.6 | 239 | 1.10 |
| | 3 | 233 | 218 | 1.07 | 236.8 | 214.8 | 1.10 |
| C | Control | 267.3 | 258.6 | 1.03 | 280.6 | 269 | 1.04 |
| | 3 | 265.6 | 250.6 | 1.06 | 266.1 | 260.8 | 1.02 |
| D | Control | 249.6 | 238 | 1.05 | 269.3 | 247 | 1.09 |
| | 3 | 224.5 | 201.1 | 1.11 | 220.2 | 195.8 | 1.12 |
| E | Control | 214 | 198 | 1.07 | 205.3 | 193 | 1.06 |
| | 3 | 208.1 | 202.8 | 1.02 | 200.4 | 190.2 | 1.05 |
| F | Control | 181.6 | 174.3 | 1.04 | 192 | 177 | 1.08 |
| | 3 | 172.1 | 158.5 | 1.09 | 172.5 | 159.6 | 1.08 |

| | *Tests made 6 to 8 hours after end of exposure period** | | | | | |
|---|---|---|---|---|---|---|
| Subject | After exposure to gas | | | Control | | |
| | First | Last | Ratio | First | Last | Ratio |
| A | 239.3 | 247.3 | 0.96 | 262.3 | 257.3 | 1.01 |
| B | 259.6 | 249.6 | 1.04 | 269.6 | 251.3 | 1.07 |
| C | 275.3 | 264.3 | 1.04 | 288.6 | 282 | 1.02 |
| D | 242.6 | 237.3 | 1.02 | 268.3 | 256 | 1.05 |
| E | 244.6 | 241.3 | 1.01 | 218 | 206 | 1.06 |
| F | 208 | 191.6 | 1.08 | 189.6 | 185.6 | 1.02 |

*Approximately 9 to 10 P.M.

### *First and Last Five Intervals in Tapping Test*

Table 2 shows the average scores for the first and last 5 (10 seconds) intervals for each subject after exposure to 3 and 4 parts of CO in 10,000 of air, and also under control conditions. In this test, as in the dynamometer test, the averages are not directly comparable because of the practice effect. This condition holds true of all the tests used. A comparison of the ratios for the different work-

ing conditions shows that even though a difference exists between the ratios, it seems to be a chance difference, because all of the 6 subjects do not show a difference in the same direction. If there were some deleterious effect due to the amount of CO in the cham-

TABLE 2

PERFORMANCE IN TAPPING TEST

| Subject | CO parts/ 10,000 | *Averages for first and last 5 (10 seconds) periods* A.M. First | A.M. Last | Ratio | P.M. First | P.M. Last | Ratio |
|---|---|---|---|---|---|---|---|
| A | Control | 263.7 | 255.5 | 1.03 | 256.3 | 241.5 | 1.07 |
| | 3 | 270.2 | 255.7 | 1.06 | 269 | 253 | 1.06 |
| | 4 | 252.6 | 218.3 | 1.16 | 248.5 | 218.5 | 1.14 |
| B | Control | 283 | 245.2 | 1.15 | 274 | 250.6 | 1.10 |
| | 3 | 284.5 | 245.7 | 1.16 | 293 | 244.3 | 1.20 |
| | 4 | 266 | 231.3 | 1.15 | 260 | 235.5 | 1.10 |
| C | Control | 268.5 | 260.7 | 1.03 | 247.1 | 248.6 | .99 |
| | 3 | 270.2 | 253.6 | 1.06 | 274.2 | 256.2 | 1.06 |
| | 4 | 247.6 | 235.3 | 1.05 | 261.7 | 249.5 | 1.05 |
| D | Control | 290.2 | 280.7 | 1.03 | 288.1 | 276.6 | 1.08 |
| | 3 | 294.3 | 289 | 1.02 | 296.2 | 284 | 1.04 |
| | 4 | 275.6 | 259.3 | 1.06 | 286.7 | 262.2 | 1.09 |
| E | Control | 292.7 | 273.7 | 1.07 | 273.5 | 268.8 | 1.02 |
| | 3 | 297 | 279.2 | 1.06 | 296.2 | 282 | 1.05 |
| | 4 | 268.6 | 261.3 | 1.03 | 272.7 | 216.2 | 1.26 |
| F | Control | 286.7 | 262.2 | 1.09 | 275.1 | 254.6 | 1.08 |
| | 3 | 279.2 | 243.7 | 1.14 | 285.5 | 251.2 | 1.13 |
| | 4 | 274.6 | 258 | 1.06 | 280.5 | 259 | 1.08 |

ber, it might be expected that when the subjects were exposed to 4 parts there would be a greater falling off in work than when they were exposed to 3 parts. What actually happened is that three of the men show a greater falling off under 4 parts and three show greater falling off under 3 parts.

### *First and Last Five-Minute Periods in Felix Test*

Table 3 shows the average for the first and last 5-minute periods on the Felix test. In this test there are some differences between the ratio which might seem significant. If careful scrutiny is made of these differences it will be found that the differences do not favor any single condition. That is to say, all of the subjects differ as to the conditions under which they made a high and low ratio between the first and last part of the performance. The statement does not hold true for the work periods at night. Five of the six subjects show a less decrease in their work on the control nights. Subject A shows improvement during the working period on the control nights.

Table 4 shows the averages for the substitution test. Columns

TABLE 3
PERFORMANCE IN FELIX TEST

*Averages for first and last 5-minute periods*

| Subject | CO parts/10,000 | A.M. First | A.M. Last | Ratio | P.M. First | P.M. Last | Ratio |
|---|---|---|---|---|---|---|---|
| A | Control | 72.2 | 109.7 | 0.66 | 95.6 | 128.6 | 0.74 |
| | 3 | 68.6 | 91.3 | 0.75 | 67 | 84.5 | 0.80 |
| | 4 | 122.6 | 168 | 0.73 | 103.2 | 107.5 | 0.96 |
| B | Control | 43 | 71.5 | 0.60 | 104.8 | 115.6 | 0.90 |
| | 3 | 47 | 63.3 | 0.75 | 91 | 129.3 | 0.71 |
| | 4 | 81.5 | 147.7 | 0.55 | 91 | 107.5 | 0.71 |
| C | Control | 82.3 | 94.7 | 0.88 | 109.8 | 140.3 | 0.78 |
| | 3 | 89.3 | 118 | 0.76 | 97 | 130.2 | 0.74 |
| | 4 | 103 | 167.7 | 0.61 | 120.6 | 162.8 | 0.74 |
| D | Control | 92.4 | 112.2 | 0.83 | 79.2 | 120.7 | 0.66 |
| | 3 | 74 | 83.6 | 0.88 | 111 | 147.5 | 0.76 |
| | 4 | 143.5 | 168 | 0.85 | 104.2 | 142.2 | 0.73 |
| E | Control | 101 | 113 | 0.89 | 99.8 | 150.6 | 0.66 |
| | 3 | 129.3 | 167.6 | 0.77 | 160.3 | 205 | 0.78 |
| | 4 | 186.2 | 207.2 | 0.89 | 166 | 186.7 | 0.89 |
| F | Control | 95.5 | 94.7 | 1.01 | 117.3 | 132.6 | 0.89 |
| | 3 | 114.3 | 133.3 | 0.86 | 126 | 161.2 | 0.78 |
| | 4 | 179 | 214.2 | 0.83 | 159.3 | 199.3 | 0.80 |

*Tests made 6 to 8 hours after end of exposure period*

| Subject | After exposure to gas First | Last | Ratio | Control First | Last | Ratio |
|---|---|---|---|---|---|---|
| A | 54 | 79 | 0.69 | 94.6 | 74.3 | 1.28 |
| B | 57 | 97.3 | 0.57 | 22.6 | 41.3 | 0.55 |
| C | 95 | 123 | 0.77 | 94 | 116 | 0.81 |
| D | 82 | 114.3 | 0.72 | 78 | 109 | 0.72 |
| E | 152 | 176.3 | 0.86 | 133.3 | 145.6 | 0.92 |
| F | 155.3 | 207.6 | 0.73 | 155.6 | 207 | 0.75 |

marked $S_1AM$ and $S_2AM$ show the average number of correct substitutions made during the two successive work periods in the morning. The columns marked $S_1PM$ and $S_2PM$ show the averages made in the afternoon during two work periods. This table does not furnish any data that can be used as evidence for or against the effects produced by CO. The performances at night do not show any general tendency in favor or against working conditions after exposure to 3 parts CO. Curves were plotted to determine whether there was any characteristic differences in the form of the work curve or the fatigue curve. Although curves were plotted for control and for gas conditions for the dynamometer, tapping, and Felix tests, no general tendency could be noted that would enable one to distinguish the control curves from the gas curves.

TABLE 4

PERFORMANCE IN SUBSTITUTION TEST

| Subject | CO parts/10,000 | $S_1$AM | $S_2$AM | AV. AM | $S_1$PM | $S_2$PM | AV. PM |
|---|---|---|---|---|---|---|---|
| | *Averages for the first and second 5-minute work periods* | | | | | | |
| A | Control | 116.2 | 114.8 | 115.5 | 109 | 120.2 | 114.6 |
| | 2 | 96.2 | 90.5 | 93.3 | 92.1 | 97.9 | 95.0 |
| | 3 | 103 | 115.5 | 109.2 | 110 | 119.7 | 114.8 |
| | 4 | 114.6 | 116.6 | 115.6 | 112 | 92 | 102 |
| B | Control | 167.4 | 168.5 | 168 | 162.2 | 167.4 | 164.8 |
| | 2 | 153.1 | 145.8 | 149.4 | 146.9 | 154.7 | 150.8 |
| | 3 | 150.5 | 146.2 | 148.3 | 121 | 137.7 | 129.3 |
| | 4 | 168.3 | 173 | 170.6 | 158.5 | 161.2 | 159.8 |
| C | Control | 160.8 | 158 | 159.4 | 168.8 | 161.2 | 165 |
| | 2 | 136.9 | 136.3 | 136.6 | 140.1 | 143.7 | 140.9 |
| | 3 | 165 | 166 | 165.5 | 176.7 | 162.5 | 169.6 |
| | 4 | 161 | 163 | 162 | 159.7 | 166.2 | 162.9 |
| D | Control | 128.8 | 130.2 | 134.5 | 133 | 132.8 | 132.9 |
| | 2 | 120 | 116.5 | 118.2 | 122.1 | 116.6 | 119.3 |
| | 3 | 130.3 | 125.3 | 127.6 | 125 | 137.8 | 131.4 |
| | 4 | 118.6 | 125 | 120.8 | 135 | 130.7 | 132.8 |
| E | Control | 183.6 | 192.4 | 188 | 189.2 | 190 | 189.6 |
| | 2 | 160.3 | 160.7 | 160.5 | 159.7 | 161.1 | 160.4 |
| | 3 | 182.7 | 178 | 180.3 | 174.2 | 133.2 | 180.3 |
| | 4 | 188 | 194.3 | 191.1 | 181.2 | 176.2 | 178.7 |
| F | Control | 140.8 | 138.6 | 139.7 | 133 | 127.8 | 130.4 |
| | 2 | 128.5 | 122.8 | 125.6 | 123.8 | 127 | 125.4 |
| | 3 | 142.7 | 132 | 137.3 | 135.7 | 124.2 | 129.9 |
| | 4 | 137.3 | 132 | 134.6 | 125.5 | 121.7 | 123.6 |

*Tests conducted at night, after exposure*

| Subject | CO parts/10,000 | Substitution night | Substitution night | Average night |
|---|---|---|---|---|
| A | Control | 133 | 135.3 | 134.1 |
| | 3 | 123.3 | 127.6 | 125.4 |
| | 3 | 165.3 | 186.3 | 175.8 |
| B | Control | 166.3 | 153 | 159.6 |
| C | Control | 182.3 | 186 | 184.1 |
| | 3 | 172 | 173 | 172.6 |
| D | Control | 140.3 | 138 | 139.1 |
| | 3 | 144.3 | 138 | 141.1 |
| E | Control | 196.6 | 198.6 | 131.7 |
| | 3 | 185 | 193.6 | 189.3 |
| F | Control | 144.6 | 140.6 | 142.6 |
| | 3 | 139.6 | 144.3 | 141.9 |

## AVERAGE SCORES IN FIVE TESTS

Table 5 shows the average scores for the arithmetic, the Felix, the tapping, the steadiness, and the substitution tests. The average for the arithmetic test is the average of the total number of correct

TABLE 5

AVERAGES AND MEAN VARIATIONS IN THE ARITHMETIC, FELIX, TAPPING, STEADINESS, AND SUBSTITUTION TESTS

| | Subject A | | Subject B | | Subject C | | Subject D | | Subject E | | Subject F | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Test | Average | Mean Variation | Average | Mean Variation | Average | Mean Variation | Average | Mean Variation | Average | Mean Variation | Average | Mean Variation | Time | Condition |
| Arithmetic | 47.2 | 9.4 | 95.7 | 8.5 | 73.3 | 7.6 | 69.1 | 7.1 | 71.9 | 17.1 | 79.8 | 11.7 | A.M. | |
| | 41.3 | 10.6 | 93.2 | 5.5 | 81 | 9.8 | 73.9 | 5.08 | 69.1 | 18.9 | 78.6 | 18.8 | P.M. | Gas |
| | 66.2 | 10.8 | 88.4 | 9.1 | 91 | 5.5 | 75.4 | 5.4 | 82.2 | 6.7 | 77.2 | 8.2 | P.M. | Control |
| | 306.6 | 82.6 | 214.4 | 67.6 | 311.1 | 58.2 | 329.9 | 92.9 | 447.4 | 117.6 | 397.1 | 121.7 | A.M. | |
| Felix | 255.7 | 61.05 | 317 | 104.6 | 358.7 | 51.1 | 348.8 | 78 | 504.4 | 89.5 | 452.1 | 105.6 | P.M. | Gas |
| | 306.8 | 82.6 | 309.6 | 106.06 | 359 | 93.3 | 282.4 | 11.06 | 355.8 | 89.1 | 348.8 | 75.2 | P.M. | Control |
| | 604.6 | 29.7 | 624.4 | 25.7 | 624.3 | 26.6 | 674.3 | 22.9 | 667 | 22.1 | 634.2 | 17.6 | A.M. | |
| Tapping | 587 | 37 | 607 | 23.2 | 614.1 | 22.8 | 668.5 | 27 | 659.2 | 34.2 | 642.7 | 11.9 | P.M. | Gas |
| | 596.8 | 52.1 | 622.8 | 51.8 | 599.4 | 41.05 | 662.1 | 33.2 | 73.5 | 63.1 | 11.5 | 7.7 | A.M. | |
| Steadiness | 38.3 | 28.4 | 31.7 | 22.4 | 19.4 | 14.5 | 44.3 | 60.1 | 649.5 | 62.1 | 628.8 | 34.8 | P.M. | Control |
| | 43.2 | 24.2 | 22.2 | 13.1 | 31.3 | 23.2 | 52.5 | 32.1 | 70.6 | 44.4 | 11.9 | 8.0 | P.M. | Gas |
| | 40 | 30 | 18.8 | 15 | 12.6 | 8.1 | 52.6 | 38.4 | 49.5 | 45.1 | 22.6 | 15.2 | P.M. | Control |
| Substitution | 203.3 | 21.5 | 300.3 | 37.1 | 290.3 | 35.4 | 238.7 | 22.9 | 338.7 | 38.05 | 258.0 | 19.7 | A.M. | |
| | 201.8 | 22.01 | 310.4 | 34.3 | 301.5 | 32 | 249 | 20.3 | 329.5 | 19.7 | 247.2 | 19.3 | P.M. | Gas |
| | 212.8 | 31.8 | 307.8 | 32.9 | 301.5 | 47.5 | 246.5 | 32.1 | 355 | 40.3 | 246.8 | 23.2 | P.M. | Control |

TABLE 6

STEADINESS TEST

| Subject | CO parts/10,000 | *Average number of contacts made in 1½ minutes* Morning | Afternoon | Night* |
|---|---|---|---|---|
| A | Control | 17.8 | 16.6 | 33 |
| | 3 | | 20.8 | 18 |
| B | Control | 22.8 | 3.3 | 10.3 |
| | 3 | | 21.7 | 15.6 |
| C | Control | 6.8 | 7 | 9.3 |
| | 3 | | 13 | 13.6 |
| D | Control | 37.2 | 23.6 | 27.3 |
| | 3 | | 56.6 | 37 |
| E | Control | 16.1 | 18 | 16.3 |
| | 3 | | 37 | 19.3 |
| F | Control | 9.3 | 7 | 12.3 |
| | 3 | | 17.2 | 14 |

*Test made 6 to 8 hours after exposure period.

additions in a working period of 6 minutes. The average in the Felix test is the average of the number of late responses in a work period of 14 minutes. The average for the tapping test is the average of the total number of taps made on the double tapping plate during 2 minutes. The average in the steadiness test is obtained from the averages of the total number of contacts made from the 5 holes tested, the time for each hole being 20 seconds. The average for the substitution test is the average of the total number of correct substitutions made for a working period of 10 minutes.

All of these tests show that there is no general tendency for the performance to be better or worse for any given working condition. Even in those cases where five of the six subjects made their poorest score when subjected to gas, the mean variation of the averages is so large as to make the difference between the averages unreliable.

Table 6 shows the average number of contacts made by each subject on the steadiness test using the hole (8/64 inch in diameter). The time allowed for this test was 1½ minutes. This increase in time was given to see whether any reliable difference could be obtained between control and gas periods by a more prolonged work period. As so few performances could be obtained under this condition only the averages are given. Five of the six subjects did better in the morning than they did in the afternoon, and all six subjects did better on the control afternoon than they did on the gas afternoons. These averages show only the tendency and cannot be considered as statistically reliable. If the averages made at night after exposure to gas during the day are compared with the averages

of the control nights, it will be found again that five of the six subjects made a poorer score on the gas nights than they did on the control nights. These figures also cannot be relied upon statistically.

*Tests for Color Blindness and Visual Acuity*

Tests were made for color blindness and visual acuity after exposure to exhaust gas. All of the subjects were found to be normal on these tests.

## Conclusions

It might be concluded that no significant difference in performance was found after exposure to exhaust gas-air mixtures which contained 2, 3, and 4 parts of CO per 10,000 of air for periods ranging from 3 to 5 hours and which caused 25 to 35 per cent of the hemoglobin to be in combination with CO. Although there was a slight tendency for a poorer performance to be made on the prolonged steadiness test, the difference in performance on all the other tests used was no greater than the variation in performances on successive control periods.

This paper can only emphasize the necessity for more work in devising tests that are adequate for measuring fatigue. If the tests which have been described are to be used in work of this nature, it is quite essential that the period allowed for testing be increased. More subjects, both in a control group and in the experimental group, are necessary. It is possible that the tests now available would show some difference in performance if the last two conditions mentioned are satisfied.

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## L'EFFET DU MONOXYDE DE CARBONE SUR LE RENDEMENT DANS DE CERTAINS TESTS PSYCHOLOGIQUES

(Résumé)

On a fait cette expérience dans le but de déterminer l'effet du gaz monoxyde de carbone sur le rendement dans de certains tests psychologiques. L'accroissement du nombre d'automobiles et l'accroissement résultant du gaz monoxyde de carbone a donné à cette expérience une importance pratique.

On a exposé six sujets au gaz monoxyde de carbone d'une machine à pétrole pendant environ deux mois et demi, les périodes de test variant de 2½ à 6 heures par jour, sauf certains jours de contrôle. Pendant ce temps on leur a fait subir les tests psychologiques suivants: le tapping, le steadiness, le dynamomètre, le calcul, la mémoire des places, la substitution et une forme de test de réaction continue.

On a fait une comparaison des rendements après qu'on avait exposé les sujets à 2, 3, et 4 parties de CO pour 10,000 d'air, avec les rendements dans les conditions de contrôle.

On n'a trouvé aucunes différences signifiantes dans les différentes conditions de travail. Il est essentiel qu'on fasse d'autres expériences pour qu'on puisse formuler de bons tests psychologiques pour la mesure de la narcose de la fatigue.

DORCUS ET WEIGAND

## DIE WIRKUNG VON AUSPUFFGAS AUF DIE LEISTUNGEN BEI GEWISSEN PSYCHOLOGISCHEN UNTERSUCHUNGEN

(Referat)

Der Zweck dieser Untersuchung war die Wirkung von Auspuffgas auf die Leistungen bei gewissen psychologischen Tests zu ermitteln. Die Zunahme der Automobile und folglich die Zunahme von Auspuffgas hat dieser Frage praktische Wichtigkeit verliehen.

Die 6 Vpn wurden 2½ Monate lang, täglich 2½ bis 6 Stunden der Einwirkung des Auspuffgases ausgesetzt, ausser an bestimmten Kontrolltagen. Während dieser Zeit wurden sie den folgenden Tests unterzogen: Motorik, Unerschütterlichkeit, Dynamometer, Rechnen, Orientierungsgedächtnis, Substitution, oder einer Art anhaltender Reaktionstests.

Nachdem man die Vpn dem Auspuffgas ausgesetzt hatte—2, 3, und 4 Teile Co auf 10,000 Teile Luft—verglich man ihre Leistungen mit den Kontrolleistungen.

Man fand keine bedeutsamen Unterschiede bei verschiednen Arbeitsumständen. Es ist von Wichtigkeit, dass weitere Versuche gemacht werden um passende psychologische Tests zur Bemessung der Narkose oder Ermüdung auszuarbeiten.

DORCUS UND WEIGAND

## ВЛИЯНИЕ ОТРАБОТАННОГО ГАЗА НА ВЫПОЛНЕНИЕ НЕКОТОРЫХ ПСИХОЛОГИЧЕСКИХ ТЕСТОВ.

(Реферат).

Задачей этого эксперимента было выяснить влияние отработанного газа на выполнение некоторых психологических тестов. Все увеличивающееся количество автомобилей и соответственное увеличение отработанного газа делает такое исследование практически необходимым.

Шесть человек были подвергнуты влиянию отработанного газа, полученного от машины, приводимой в движение газолином в течение $2^1/_2$ месяцев, ежедневно от $2^1/_2$ до 6 часов, за исключением некоторых контрольных дней. В течение этого времени им были пред'явлены следующие психологические тесты: постукивание, работа с динамометром, арифметические задачи, память на места и некоторые формы теста с продолжительной реакцией.

Были сравнены выполнения тестов при пред'явлении 2, 3 и 4 частей окиси углерода на 10.000 частей воздуха и при обычных контрольных условиях. Эти различные условия не дали заметной разницы. Необходимо производить дальнейшие опыты для выработки адекватных психологических тестов для измерения влияния наркотических средств и усталости.

**Доркус и Вайганд (Dorcus and Weigand).**

# JACQUES LOEB: A CONTRIBUTION TO THE HISTORY OF PSYCHOLOGY*

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DOROTHY E. PALMER

As modern psychology adopts experimental methods, psychological theory is developing in the direction of biological principles and social statistics. The work of Jacques Loeb is particularly important as a basis for the biological aspect of psychological theory and experimentation, since he presents a large body of experimental work which bears directly upon such important topics as the nature of instincts, consciousness and will, heredity, environmental influences, and the processes underlying stimulation and inhibition.

Jacques Loeb was born in Germany in 1859 and died in 1924. According to Simon Flexner (1927), "His forebears were among the intellectuals who during the persecutions attendant on the Inquisition were driven out of Portugal and compelled to seek asylum in a more liberal country. They migrated from Lisborn to Amsterdam, and in later and quieter times settled in Alsace" (p. 334). In the French-German environment, Loeb became interested in the writings of the French philosophers of the first revolution. To d'Alembert, Diderot, Holbach, and Voltaire he dedicated his book, *The Organism as a Whole* (1916), referring to them as "that group of freethinkers . . . who first dared to follow the consequences of a mechanistic science—incomplete as it then was—to the rules of human conduct . . ." (p. viii).

The scientific training which Loeb received while a medical student in the German universities directed his energies into experimental studies, as Flexner points out. At Strassburg, Loeb studied under Goltz, the pupil of Helmholtz, who was experimenting on the effect of brain and spinal cord excisions in the frog and dog, with results which contributed to Loeb's theory of consciousness as developed in the *Comparative Physiology of the Brain and Comparative Psychology* (1900).

At Würzburg, while assisting the physiologist Fick, Loeb made a chance friendship with the botanist Sachs, who was developing plant

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physiology on the basis of physical and chemical reactions which he called tropisms, the reactions to light, chemicals, and gravity being named respectively heliotropism, chemotropism, and geotropism. The importance of similar studies on animal behavior was impressed upon Loeb.

Comparative psychology at that time was in the hands of a group of writers described by C. J. Warden (1927) as collectors of stories. Their anecdotes were given as proof of a rudimentary human mind in the higher animals (p. 147). The evolutionary theory advanced by Darwin had led to an emphasis on the continuity of mental life between man and animals. The behavior of animals was humanized and said to have the same rational, emotional, and ethical basis as human conduct (p. 147). According to Romanes, the moth flew into the flame because of its *curiosity*.

The result of Loeb's studies led him to explain the moth's activity as forced movements resulting from a series of physico-chemical reactions. The flight into the flame conformed to a definite formula and required no psychic factor such as curiosity. As he stated his principle (1900), "the 'instinct' that drives animals into the light is nothing more than the chemical—and indirectly the mechanical—effect of light. . . Animals that move rapidly (like the moth) get into the flames before the heat of the flame has time to check them in their flight" (p. 181). His more general assumption (1906) that "Living organisms may be called chemical machines" (p. 1), included all of the processes of living organisms within the scope of biological experimentation and explanation by physico-chemical laws. For Loeb, there were no phenomena of organization or development which required the "invisible guide" assumed by Claude Bernard (1885, pp. 22-64), or the "entelechy" sponsored by Hans Driesch (1908, p. 143).

The reliance of Loeb upon the discoveries in physics and chemistry is shown in his statement of the aims and problems of his science.

In 1916 he states, "The physical researches of the last ten years have put the atomistic theory of matter and electricity on a definite and in all probability permanent basis. . . This permits us to state as the ultimate aim of the physical sciences the visualization of all phenomena in terms of groupings and displacements of ultimate particles, and since there is no discontinuity between the matter constituting the living and non-living world the goal of biology can be expressed in the same way" (p. 1).

A similar dependence of psychological theory upon physical researches is shown in the adoption by A. P. Weiss (1925) of the electron-proton hypothesis as his fundamental assumption and the statement, "All movements in the universe that have occurred in

the past, that are occurring now or that will occur in the future, may be regarded as belonging to a closed system of interconnecting forces, and the movements of mankind may be regarded as a part of this interrelated system of movements" (p. 35).

## Tropisms

In his experiments upon the reactions of organisms to light, chemicals, and gravity, Loeb (1904) followed the method outlined by him as follows:

"The problem of a scientific investigator can always be reduced to two tasks: the first, to determine the independent variables of the phenomena which he has under investigation, and, secondly, to find the formula which allows him to calculate the value of the function for every value of the variable . . . I am inclined to believe that we possess in the tropisms and tropism-like mechanisms the independent variable of such functions as the instinctive selection of food and similar regulatory phenomena" (pp. 13-24).

A tropism is defined (1906) as "The automatic orientation in a field of force toward or away from the center of force. . ." (p. 5).

The symmetrical arrangement of the muscles and sense-organs of the body are of primary importance in such orientations, according to Loeb (1918). "The significance of this symmetrical structure lies in the fact that. . . under normal conditions, the tension in symmetrical muscles is the same, and. . . the chemical constitution and the velocity of chemical reactions are the same for symmetrical elements of the surface of the body, e.g., the sense-organs" (p. 13).

"Normally the processes inducing locomotion are equal in both halves of the central nervous system, and the tension of the symmetrical muscles being equal, the animal moves in a straight line . . . If, however, the velocity of chemical reactions in one side of the body, e.g., in one eye of an insect, is increased. . . the equality of tension of the symmetrical muscles no longer exists . . . The animal will thus be compelled to change the direction of its motion and to turn to the source of light" (p. 14) . . . "The processes in the muscles may possibly consist in the establishment of a definite hydrogen ion concentration" (p. 18).

Animals which are compelled to turn and move to the source of light Loeb calls positively heliotropic. The positively heliotropic caterpillars of *Porthesia chrysorrhoea* or the winged plant lice of *Cineraria* or the newly hatched larvae of the barnacle were used in the experiments. The larvae of *Porthesia* when placed in a test tube which was at right angles to the window, moved to the window and remained there. If the test tube was turned an angle of 180°, the animals moved again to the window.

"That these animals do not go to the light because they prefer light to darkness, but because the light orients them is proved by the fact that they will go from light into the shade, if by so doing they remain oriented with their heads towards the source of light" (p. 49). For this experiment, daylight entered the upper half of the window directly, but was diffused through the lower half. In travelling toward the source of light, the animal passed first through the direct daylight, then into the diffused light.

The reactions of the positively heliotropic animals to light were found to conform to the photochemical law of Bunsen and Roscoe.

"The law of Bunsen and Roscoe says that within certain limits the chemical effect produced by light increases in proportion with the product of intensity into the duration of illumination, e.g., *Effect* = K*it*, where *i* is intensity, *t* duration of illumination, and K a constant. This is true for the blackening of photographic paper by light, and it can be shown that the same law holds for heliotropic reactions of plants as well as animals" (p. 83).

Not only could the actions of positively and negatively heliotropic animals be predicted and controlled when variations in the two factors, intensity and duration of the light, were introduced, but a reversal in sensitivity to light could be produced. Positively heliotropic animals could be made negatively heliotropic and vice versa, with changes in the chemical composition of the medium, temperature or light, depending on the species studied.

As a result of his quantitative physico-chemical studies of animal behavior, Loeb criticizes as loose and inexact current views of the sequence of events in behavior. "In many cases the problem of animal conduct is treated in a way which corresponds more to the viewpoint of the introspective psychologist than to that of the physicist. The attempts to explain animal conduct in terms of 'trial and error' or of vague 'physiological states' may serve as examples. None of these attempts have led or can lead to any exact quantitative experiments in the sense of the physicist. Other biologists have still more openly adopted an anthropomorphic method of explanation . . . such as 'selection of random movements' which, as far as the writer can see, cannot even be expressed in the language of the physicist . . . And finally since we know nothing of the sentiments and sensations of lower animals, and are still less able to measure them, there is at present no place for them in science . . . the Aristotelian viewpoint still prevails to some extent in biology, namely, that an animal moves only for a purpose, either to seek food or to seek its mate or to undertake something else connected with the preservation of the individual or the race . . . The analysis of animal conduct only becomes scientific in so far as it drops the question of purpose and re-

duces the reactions of animals to quantitative laws. This has been attempted by the tropism theory of animal conduct" (pp. 16-18).

Loeb (1900) takes up the relations between the various elements concerned in reactions, such as the irritable elements, nerves and muscles. "Experiments on the lowest animal forms have taught us that the peculiar reactions of animals are determined, first, by the different forms of irritability of the elements composing the tissues, and second by the arrangement of the muscle fibers. The central nervous system does not control response to stimulation; it merely serves as a conductor from the point of stimulation to the muscle through which weaker stimuli may pass, and pass more rapidly than would be possible if the muscle were stimulated directly" (p. 101).

He cites experiments to show that coordinated activities in medusae are not due to specific morphological structures of the ganglion cells. In ascidians, typical reflexes continue without the ganglion cells and in higher forms the iris contracts when light strikes it after the nerves have been extirpated. Loeb gives these examples to show that stimulation goes directly from muscle element to muscle element directly, or may act directly upon the muscle (p. 37).

The rôle of the nervous elements in coordinated activity is demonstrated in the arthropod *Limulus*. "The number of the respirations produced is the same even when the abdominal ganglia are isolated . . . probably due to the fact that the number of respirations is determined by the temperature and the chemical nature of the blood." The phase of movement is only the same in those gills whose ganglia are not separated from each other. "Were there a center of coordination in any ganglion, a group of ganglia separated from this center would be active in an uncoordinated manner, but such is never the case . . . The ganglion that acts first, that is to say, the ganglion that acts quickest, stimulates those connected with it nervously and so determines the correspondence of phase . . . In higher animals, the conditions controlling respiration scarcely differ from those in Limulus" (pp. 108-109).

With this explanation of the dominance of the centers of higher activity, Loeb's work may be related to that of Child (1924) on physiological gradients. His insistence on the segmental character of nervous activity, nerves serving only as conductors, as opposed to the "center" theory of the central nervous system, may be related to the work of modern investigators, as Lashley (1926), on cerebral localization.

In an animal of the level of the frog, then, "the more complicated instincts are for the most part nothing more than series of segmental reflexes . . . chain reflexes . . . The segmental reflex in the frog is,

however, determined also by the irritability of the peripheral organs," which irritability is modified by all conditions which bring about chemical changes in the body (temperature, food, sexual products).

Such a mechanistic conception of instincts may be compared with the teleological view of McDougall (1923), who says, "We may properly call 'instinctive' those actions of animals which seem to be purposive (i.e., exhibit the marks of behavior) and which are performed by any animal independently of previous experience of similar situations" (p. 71). The marks of behavior include "a certain spontaneity of movement."

Loeb states, "Our ultimate aim in the analysis of instincts is to find out by which physical and chemical properties of protoplasm they are determined . . . by varying the instincts of an animal at desire" (p. 198). In studying hunger, for instance, Loeb would measure such conditions as the effect of glycogen in the blood on hunger contractions, whereas a "purposive approach" would study "driving adjustments" such as those given by Tolman (1922).

Loeb concludes, "The analysis of other instincts, for instance, the migratory instincts of animals, leads to the same result as the analysis of the protective instincts. The instincts are not functions of certain localized 'centers', but of irritabilities of certain peripheral structures and of the connection of the same with the muscles, whereby the central nervous system only serves as a protoplasmic connection" (p. 190).

## Consciousness

Loeb (1900) in his thoroughgoing application of physico-chemical principles to the explanation of life-processes finds a mechanistic basis for such phenomena as are usually called psychic or conscious.

"The most important problem in the physiology of the central nervous system is the analysis of the mechanisms which give rise to the so-called psychic phenomena. The latter appear invariably as a function of an elementary process, namely, the activity of the associative memory . . . By associative memory I mean the two following peculiarities of our central nervous system: First, that processes which occur there leave an impression or trace by which they can be reproduced even under different circumstances than those under which they originated. This peculiarity can be imitated by machines like the phonograph . . . The second peculiarity is, that two processes which occur simultaneously or in quick succession will leave traces which fuse together, so that if later one of the processes is repeated, the other will necessarily be repeated also. The odor of a rose will at the same time reproduce its visual image in our memory . . ." (p. 213).

"I think it can be shown that what the metaphysician calls consciousness are phenomena determined by the mechanism of associative memory" (p. 214). "For the present we can say that if an animal can learn, that is, if it can be trained to react in a desired way upon certain stimuli (signs), it must possess associative memory. The only fault with this criterion lies in the fact that an animal may be able to remember (and to associate) and yet may not yield to our attempts to train it" (p. 218).

Loeb emphasizes the importance of distinguishing between reflex response to specific stimuli and true association or learning. He limits the distribution of associative memory to most of the mammals and some of the invertebrates (p. 219), among these, bees and ants. Its functioning depends on a certain degree of development of the brain (p. 252).

Loeb's view of consciousness is criticized by students of animal behavior, Margaret Washburn (1917) stating the objection that "learning by experience, taken by itself, is too indefinite a concept to be of much service" (p. 31) and "It is a question rather of the rapidity and of the kind of learning involved . . . The fact is that while proof of the existence of mind can be derived from animal learning by experience only if the learning is very rapid, other evidence, equally valid on the principle of analogy, makes it highly improbable that all animals which learn too slowly to evince the presence of ideas are therefore unconscious" (p. 34).

The processes known as will and conscious sensation are described by Loeb as functions of the mechanism of associative memory.

"In volition three processes occur. The one is an innervation of some kind which may be caused directly or indirectly by an external stimulus. This process of innervation produces two kinds of effects. The one effect is the activity of the associative memory which produces the sensations that in former cases accompanied or followed the same innervation. The second effect is a coordinated muscular activity. It happens that in such cases the reaction-time for the memory-effect of the innervation is shorter than the time for the muscular effect. When some internal process causes us to open the window, the activity of the associative memory produces the idea of sensations which will follow or accompany the opening of the window sooner than the act of opening really occurs. As we do not realize this . . . we consider the memory-effect of the innervation as the cause of the muscular effect. The common cause of both effects, the innervating process, escapes our immediate observations as our senses do not perceive it. The will of the metaphysicians is then clearly the outcome of an illusion due to the necessary incompleteness of self-observation" (p. 216).

The memory-effect for Loeb is not a localized psychic process occurring in the brain, though he considers the cerebral hemispheres necessary for the process of associative memory. "From the fact that the different bundles of fibers from the various segmental ganglia enter at different spots in the cortex, some authors have drawn the conclusion that there is not only an anatomical localization of fibers, but also a psychic localization of functions. They assume that the various psychic functions take place in different regions of the cortex . . .These assumptions are contradicted by the plain facts of associative memory. Simultaneous processes in different sense-organs are fused in our memory. The odor of a rose recalls its visual image. This could not be possible if the visual processes were confined to one region of the cerebral hemispheres; they must spread to the olfactory region, and *vice versa* . . . Moreover, we remember not only simultaneous sense-impressions, but we remember a whole series dependent upon successive stimuli of different character, if only the first constituent of such a series has been aroused. This indicates that even the after-effects of a stimulus must spread all over the cerebral hemispheres, so that they may fuse with the successive processes going on in the brain. It is thus obvious that the assumption of a localization of psychic functions in the cortex is opposed to the elementary facts of associative memory or consciousness" (p. 261).

The relation of the cerebral hemispheres to the behavior of the individual is illustrated in the case of the frog. After quoting experiments made by Goltz on extirpation of the cerebral hemispheres in this animal, Loeb concludes: "The normal frog as well as the frog without cerebral hemispheres is a reflex animal—that is, its reactions are chiefly segmental reflexes. But there is this difference between the two: In the animal with the cerebral hemispheres the same stimulus can produce more than a single reflex, and this fact adds to the greater complication and capriciousness of the reactions of the animal. On the other hand the cerebral hemispheres can also restrict the play of the segmental reflexes" (p. 238).

## Heredity

In discussing the factors involved in heredity, Loeb (1900) considers two possible views of the transmission of hereditary characters, first that all of the structures of the full-grown animal are contained in the germ cells, or, second, that the mature animal is a complex structure built up from simple chemical substances found in the germ cells. He adopts the latter view, concluding that "the sum of the elements which we call the form and instincts of the full-grown animal is only the resultant of a few simpler elements which can

readily be transmitted through the egg without its possessing a complicated structure" (p. 202).

According to this theory, heliotropic reactions are determined by the presence in the surface of the animal of specific photosensitive substances. The symmetrical structure of the animal, the second important factor in Loeb's explanation of tropisms, is also determined by simple chemical substances transmitted through the egg. "The egg is not the bearer of the form of the full-grown animal, but of certain chemical substances, especially of ferments. According to the stereochemical configurations of the latter, the products of assimilation, and with these the materials of the body, turn out differently. The process of development is not only a morphological but a chemical differentiation, and new combinations of substances are continually formed from the original raw material. A further differentiation of the form may be and often is connected with every metabolic differentiation of the substance of the body" (pp. 202-203).

The egg is considered by Loeb (1916) to be the bearer of the genus and species heredity, since it was possible to produce normal sea urchins and frogs and raise them to the adult state by artificial activation of the eggs, no spermatozoa being present. Experiments of hybridization showed that no chromosome influence was noticeable in the early development and that the nucleus had nothing to do with the determination of the embryo. These observations lend support to the view that, "the unity of the organism is not due to a putting together of a number of independent Mendelian characters according to a 'pre-established plan,' but to the fact that the organism in the rough existed already in the cytoplasm of the egg before the egg was fertilized. The influence of the hereditary Mendelian factors or genes consisted only in impressing the numerous details upon the rough block and in thus determining its variety and individuality; and this could be accomplished by substances circulating in the liquids of the body . . ." (p. 152).

In regard to the variations which are explained by various evolutionary theories, Loeb (1900) supports the idea of a discontinuous series of changes or mutations as set forth by DeVries. "The idea of a steady, continuous development is inconsistent with the general physical qualities of protoplasm or colloidal material. The colloidal substances in our protoplasm possess critical points . . . The colloids change their state very easily, and a number of conditions—temperature, ions, enzymes—are able to bring about a change in their state. Such material lends itself very readily to a discontinuous series of changes, while a gradual steady development, such as most Darwinians assume, is practically excluded" (p. 253).

Loeb shows by various analogies that constant conditions can pro-

duce discontinuous effects. "If a small, constant stream of water flows into a pipette, it will pass out rhythmically in drops" (p. 21). This principle of discontinuous change he applied to many life-phenomena, such as mutations and rhythmical activity of muscles and nerves.

Various cases are pointed out (1916) where the usually accepted Lamarckian view of adaptation to environmental conditions must be replaced by the mutation theory. Positive heliotropism exists in animals that have no opportunity to make use of it, such as crustaceans living in the mud, and the caterpillars of the willow borer living under the bark of trees. Many organisms show positive and negative responses to a constant electric current, yet are never exposed to it outside of the laboratory. The question of use and disuse has nothing to do with the production of the chemical compounds in the body upon which tropisms depend (p. 319). "The process, as popularly conceived, is the reverse of the truth; those survive that have the equipment,—they did not acquire the equipment under the influence of environment" (p. 343).

"A famous case for the apparent adaptation of animals to environment has been the blind cave animals" (p. 319). The fact that blind salamanders, blind fishes, and blind insects are common in caves, but rare in the open suggested the idea that the darkness of the cave was the cause of the degeneration of the eyes. In experiments on the fish *Fundulus* "The writer tried repeatedly, but in vain, to produce *Fundulus* with deficient eyes by keeping the embryos in the dark" (p. 322). Loeb showed that defective circulation occurred in all cases where the eyes degenerated. "All the facts agree that conditions which lead to an abnormal circulation (and consequently also to an abnormal or inadequate nutrition of the embryonic eye) may prevent development and lead to the formation of blind fishes" (p. 323). The prevalence of blind forms in caves is explained by the fact that they are all negatively heliotropic and positively stereotropic. "In other words, the adaptation of blind animals to the cave is only apparent; they were adapted to cave life before they entered the cave" (p. 325). The adaptations themselves are mutations.

The apparent adaptation of an animal, then, is explained by its original inherited flexibility, whereby it was capable of living under conditions which it might never meet. Experiments on the fish *Fundulus* showed that beyond the limits of immunity to change already possessed by the fish, there was no adaptive effect. Loeb quotes cases of apparent adaptation in the formation of the lens, in the activating effect of the spermatozoön on the egg, and in the exposure of the stems and leaves of plants to sunlight. He states

that there are just as many cases where chemical substances circulating in the body lead to indifferent or harmful results. As an example of the indifferent type, he mentions the existence of heliotropism in animals living in the dark; of the harmful type, the inheritance of deficiencies like color-blindness or glaucoma.

Loeb concludes that "While it is possible for forms with moderate disharmonies to survive, those with gross disharmonies cannot exist and we are not reminded of their possible existence . . . It is possible to fertilize the eggs of each marine bony fish with the sperm of practically every other marine bony fish . . . If we accomplish all possible hybridizations, one hundred million different crosses will result. Of these only a small fraction of one percent can live, and it is generally the lack of a proper circulation which inhibits them from reaching maturity . . . If we consider these facts we realize that the mere laws of chance are adequate to account for the fact of the apparently purposeful adaptations; as they are adequate to account for the Mendelian numbers" (pp. 344-345).

## Environmental Factors

The relation of an animal to environmental factors is pointed out by Loeb (1912) in the case of the dependence of life-processes upon oxygen. "Individual life begins (in the case of the sea urchin and possibly in general) with the acceleration of the rate of oxidation in the egg, and this acceleration begins after the destruction of its cortical layer. Life of warm-blooded animals—man included—ends with the cessation of oxidation in the body . . . The problem of the beginning and end of individual life is physico-chemically clear" (p. 14).

In discussing the various factors of the environments, Loeb (1916) follows his usual procedure of reducing them to quantitative laws. "If we substitute for the indefinite term environment the individual physical and chemical forces which constitute environment it is possible to show that the influence of each of these forces upon the organism finds its expression in simple physicochemical laws and that there is no need to introduce any other considerations" (p. 286).

Temperature, light, chemical agencies, and gravitation are among the influences studied. "The influence of temperature upon development has been used to find the conditions determining fluctuating variation . . . the differences between individuals of a pure strain or breed. These variations are not inherited, a fact contrary to the idea of Darwin, who assumed that by the selection of extreme cases of fluctuating variation new varieties could develop" (pp. 296-297).

The results of experiments showed that temperature affects phenomena in the same way as it does the velocity of chemical reactions,

showing the continuity of chemical conditions in animate and inanimate matter. "The point of importance to us is that the influence of temperature upon the organism is so constant that if disturbing factors are removed it would be possible to use the time of insemination to the first segmentation of an egg of *Arbacia* as a thermometer . . . Facts of this character should dispose of the idea that the organism as a whole does not react with that degree of machine-like precision which we find in the realm of physics and chemistry" (p. 299).

In comparing the effects of changes in chemical composition of the surrounding medium and changes in the temperature, Loeb (1905) found the following relations: "Through an increase in the concentration of the sea-water the same results can be obtained as through a lowering of the temperature. Negatively heliotropic larvae become positively heliotropic, and positively heliotropic larvae become still more positive. Through a decrease in the concentration of the sea-water the same effect is obtained as through an increase in its temperature. The positively heliotropic animals become negatively heliotropic, and the negatively heliotropic animals more strongly negative" (p. 279).

The importance of these responses is pointed out in such forms of behavior as the depth migrations of fishes. Many forms living near the surface of the ocean or fresh water migrate upward toward evening and downward toward the end of the day. Loeb (1912) analyzes the behavior as follows: "Since the consumption of carbon dioxide by the green plants ceases toward evening, the tension of this gas in the water must rise and this must have the effect of inducing positive heliotropism or increasing its intensity. At the same time the temperature of the water near the surface is lowered and this also increases the positive heliotropism in the organisms" (p. 221).

Loeb's study of the effect of changes in the chemical constituents of the environment led him to conclude that the maintenance of a certain chemical equilibrium in the medium in which the cells of an organism live is one of the essential conditions for the maintenance of life, the ratio of the Na, K, and Ca salts being of the greatest importance. His analysis of the action of these antagonistic salts is summed up (1916) in the "theory of physiologically balanced salt solutions, by which we mean that in the ocean (and in the blood or lymph) the salts exist in such ratio that they mutually antagonize the injurious action which one or several of them would have if they were alone" (p. 317). The ratio of Na plus K salts to the Ca plus Mg salts must remain within definitely determined limits if the organism is to survive. Such evidence leads to the conclusion that "It is obvious that the quantitative laws prevailing in the effect of environ-

ment upon organisms leave no more room for the interference of the 'directing force' of the vitalist than do the laws of the motion of the solar system" (p. 317).

## Stimulation—Activation and Inhibition

His study of the effects of the antagonistic salts led Loeb (1906) to the conclusion that what is generally called irritability and contractility is due to the influence of Na, Ca, K, and Mg ions (p. 77). He outlined his general theory of irritability (1899) as follows: "The salts, or electrolytes in general, do not exist in living tissues as such exclusively, but are partly in combination with proteids (or fatty acids). The salts or electrolytes do not enter into this combination as a whole, but through their ions. The great importance of these ion-proteid compounds (or soaps) lies in the fact that, by the substitution of one ion for another, the physical properties of the proteid compounds change (e. g., their surface tension, their power to absorb water, or their viscosity or state of matter). We thus possess in these ion-proteid or soap compounds essential constituents of living matter, which can be modified at desire, and hence enable us to vary and control the life phenomena themselves . . . Life phenomena, and especially irritability, depend on the presence in the tissues of a number of various metal proteids, or soaps (Na, Ca, K, and Mg), in definite proportions" (1906, p. 78).

Loeb first applied his principle to the production of rhythmical contractions in skeletal muscle. It had been found that the frog's sartorius muscle could be made to contract, beating rhythmically like the heart. Loeb found "that such rhythmical contractions occur only in solutions of electrolytes, i.e., compounds which are capable of ionization . . . Only in certain, not in all, salt solutions are such rhythmical contractions possible . . . If the quantity of the Na-ions becomes too great in proportion to the Ca-ions, the muscle loses its irritability. On the other hand, if there are too many Ca-ions present, the rhythmical contractions become also impossible. The quotient of the concentration of the Na-ions over the concentration of the Ca-ions . . . becomes therefore of importance for phenomena of irritability . . . Mg acts very much like Ca in this respect . . . We owe it to the Ca- and Mg-salts in our blood that our skeletal muscles do not contract rhythmically like our heart" (1906, p. 78, 79).

The fact that different tissues are made to contract by different proportions of the antagonistic salts was shown in the case of the jellyfish. It had been observed by Romanes that if the edge of the *Medusa's* bell were cut away from the center, the edge continued to beat in normal sea-water, while the center stopped its beating.

Loeb found that "the isolated center . . . is able to beat rhythmically in a pure solution of NaCl. The center beats in such a solution very rapidly, and the more rapidly the higher, within certain limits, the concentration of the NaCl solution. The addition of a small quantity of $CaCl_2$ or $MgCl_2$ retards or inhibits the contractions caused in the NaCl solution" (p. 81).

"We come to the conclusion that all those phenomena which depend on the action of muscles, nerves, or glands seem to be influenced to a large extent by the salts, and that especially changes in the proportion of Na or K to the Ca-ions in the tissues seem to affect their properties and their actions" (p. 94).

"By the word 'stimulation' we mean a process which is unknown to us, which however, seems to consist . . . in the substitution of Na- or K-ions for Ca, or vice versa, in some colloidal (proteid or lipoid) compound of the muscle or nerve, whereby some physical qualities of the colloidal substances are changed" (p. 105).

He describes (1922) the relation between changes in the concentration of ions to electrical currents, and the dependence of differences of potential upon the presence of semi-permeable membranes. "In general the block in the diffusion of one kind of ions can be brought about by two kinds of conditions: first by membranes with a selective permeability, and second by the cohesion between ions of one kind" (p. 278).

Loeb's theory of stimulation and inhibition with its emphasis upon the physiological effects of antagonistic salts may be compared with recent theories of excitation and inhibition in the central nervous system based on antagonistic chemical effects. Fulton (1926) states the "chemical" theory as follows: "The inhibitory process might be visualized as involving a chemical process, which tends to oppose that produced by the excitatory process . . . The assumption of such depressant qualities serves as an adequate basis for explanation of all of the phenomena of reflex physiology at present recognized. It may be pointed out that the specialized excitatory and inhibitory processes, here assumed, having no refractory phase, have a parallel in the local excitatory process as defined by Lucas and Lapicque" (pp. 356-362).

Fulton further shows that such a view makes central inhibition of the same nature as peripheral inhibition, discussed by Howell (1925).

Recent discussions of psychological theory have included physicochemical assumptions of the nature of stimulation and inhibition in nerves and muscles, as in papers by Johnson (1927) and Cason (1925). Theories of inhibition, such as McDougall's theory of inhibition by drainage, and others, have been criticized on the ground

that they did not conform to experimental evidence as to the nature of nervous processes. (Bayliss, 1924, p. 424).

## Summary

Some of the main points which Loeb emphasized in his study of the behavior of organisms may be summarized as follows:

Living organisms may be called chemical machines, consisting essentially of colloidal material.

Instincts are not functions of certain localized "centers," but of irritabilities of certain peripheral structures and of the connection of the same with the muscles, whereby the central nervous system only serves as a protoplasmic connection. (Loeb's view of the mechanism of tropistic activities is criticized in some details by Mast, 1923-24.)

Associative memory is defined as the mechanism by means of which a stimulus produces not only the effects which correspond to its nature and the specific structure of the stimulated organ, but which produces, in addition, such effects of other causes as at some former time may have attacked the organism almost or quite simultaneously with the given stimulus.

Consciousness is only a metaphysical term for phenomena which are determined by associative memory.

The Mendelian factors of heredity must have the embryo to work on and the organism is not to be considered a mere mosaic of Mendelian factors. The mere laws of chance are adequate to account for the fact of the apparently purposeful adaptations; as they are adequate to account for the Mendelian numbers. Those survive that have the equipment.

Life-phenomena are affected by temperature in the same sense as the velocity of chemical reactions. Next or equal in importance with the temperature is the nature of the medium in which the cells are living.

The word "stimulation" refers to a process which seems to consist in the substitution of Na- or K-ions for Ca, or vice versa, in some colloidal compound of the muscle or nerve.

## Bibliography

Bayliss, W. M. Principles of general physiology. London: Longmans, Green, 1924.

Bernard, C. Leçons sur les phénomènes de la vie, I. Paris, 1885.

Cason, H. The physical basis of the conditioned response. *Amer. J. Psychol.*, 1925, **36**, 371–393.

Child, C. M. Physiological foundations of behavior. New York: Holt, 1924.

DRIESCH, H. The science and philosophy of the organism: the Gifford lectures, I. London: A. & C. Black, 1907.

FLEXNER, S. Jacques Loeb and his period. *Science*, 1927, **66**, 333–337.

FULTON, J. F. Muscular contraction and the reflex control of movement. Baltimore: Williams & Wilkins, 1926.

HOWELL, W. H. Inhibition. *Physiol. Rev.*, 1925, **5**, 161–181.

JOHNSON, H. M. A simpler principle of explanation of imaginative and ideational behavior and of learning. *J. Comp. Psychol.*, 1927, **7**, 187–235.

LASHLEY, K. S. Studies in cerebral function in learning: VII. The relation between cerebral mass, learning and retention. *J. Comp. Neurol.*, 1926, **41**, 1–59.

LOEB, J. Comparative physiology of the brain and comparative psychology. New York: Putnam, 1900.

————. Recent development of biology. *Int. Congress Arts & Sci.*, 1904, **5**, 13–24.

————. Studies in general physiology. 2 vols. Chicago: Univ. Chicago Press, 1905.

————. The dynamics of living matter. New York: Columbia Univ. Press, 1906.

————. The mechanistic conception of life. Chicago: Univ. Chicago Press, 1912.

————. The organism as a whole. New York: Putnam, 1916.

————. Forced movements, tropisms and animal conduct. Philadelphia: Lippincott, 1918.

————. Proteins and the theory of colloidal behavior. New York: McGraw-Hill, 1922.

MCDOUGALL, W. Outline of psychology. New York: Scribner, 1923.

MAST, S. O. Photic orientation in insects with special reference to the drone-fly, Eristalis tenex, and the robber-fly, Erax rufibarbis. *J. Exper. Zoöl.*, 1923, **38**,, 109–207.

TOLMAN, E. C. Can instincts be given up in psychology? *J. Abn. & Soc. Psychol.*, 1922, **7**, 139–152.

WARDEN, C. J. The historical development of comparative psychology. *Psychol. Rev.*, 1927, **34**, 135–168.

WASHBURN, M. F. The animal mind. New York: Macmillan, 1917.

WEISS, A. P. A theoretical basis of human behavior. Columbus, Ohio: R. G. Adams & Co., 1925.

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## JACQUES LOEB: CONTRIBUTION A L'HISTOIRE DE LA PSYCHOLOGIE

(Résumé)

Jacques Loeb (1859-1924) a développé la thèse que les organismes humains sont des machines physico-chimiques dont les actions sont capables d'une explication par des lois méchaniques. Un méchanisme, la mémoire d'association, est supposé comme base des phénomènes de la conscience. L'effet de la mémoire n'est pas un processus localisé ayant lieu dans le cerveau, bien que Loeb considère les hémisphères cerébraux nécessaires au processus de la mémoire d'association. Le tissu nerveux est seulement un tissu conducteur.

Les soi-disant activités instinctives ont été crues être des orientations automatiques d'un organisme dans un cercle de force vers ou loin du centre de force. Ces orientations ou tropismes, résultent des changements chimiques inégaux chez des organes symétriques de sens lesquels causent une tension inégale des muscles symétriques avec des mouvements tournants ou mouvements forcés. De telles réponses tropiques à la lumière, aux matières chimiques, et à la gravité obéissent aux lois quantitatives.

Les adaptations apparement délibérées sont expliquées ainsi: l'animal possède une flexibilité héritée qui le rend capable de vivre dans des conditions ordinairement jamais rencontrées.

L'étude par Loeb des effets des sels antagonistes lui a fait considérer l'irritabilité et la contractilité dues à l'influence des ions Na, Ca, K, et Mg, la stimulation apparemment consistant en la substitution des ions Na ou K pour Ca, ou vice versa, dans quelque composé colloïdal du muscle ou du nerf.

PALMER

## JACQUES LOEB: EIN BEITRAG ZUR GESCHICHTE DER PSYCHOLOGIE

(Referat)

Jacques Loeb (1859-1924) entwickelte die Theorie, dass der menschliche Organismus eine physico-chemische Maschine ist, deren Funktionen durch Gezetze der Mechanik erklärt werden können. Als Basis aller bewusten Erscheinungen, nimmt er einen Mechanismus, ein ideenverbindendes Gedächtnis an. Die Gedächtniswirkung ist kein örtlicher Vorgang, der im Gehirn stattfindet, obwohl er die Gehirnlappen für die Funktion des ideenverbindenden Gedächtnisses für notwendig hält. Das Nervengewebe ist nur Leitungsgewebe.

Die sogenannten instinktiven Fähigkeiten waren automatische Orientierungen eines Organismus in einem Gebiet der Kraftströmung, entweder zu dem Mittelpunkt dieser Kraft oder von demselben hinweg. Diese Orientierungen oder Tropismen, werden durch ungleiche, chemische Veränderungen in symetrischen Sinnesorganen verursacht, die in symetrischen Muskeln zu ungleicher Spannung führen und Wendungen oder forcirte Bewegungen hervorrufen. Solch tropistische Reaktionen gegen Licht, chemische Substanzen und Schwerkraft gehorchen quantitativen Gesetzen.

Scheinbar absichtliche Anpassungen erklärt man durch die ererbte Elastizität, Anpassungsfähigkeit des Tieres, welche dieses befähigen unter bisher ganz ungewohnten Lebensverhältnissen zu existiren.

Loeb's Studium der Wirkungen antagonistischer Salze liess ihn schliessen, dass Reizbarkeit und Kontraktibilität von Na-, Ca-, K-, und Mg-Ionen, bewirkt werden, und dass der Reiz scheinbar in dem Ersatz von Na-oder K-Ionen durch Ca—oder umgekehrt—besteht, in irgend einer Verbindung im Muskel oder Nerven.

PALMER

## ЖАК ЛЕБ: К ИСТОРИИ ПСИХОЛОГИИ.

### (Реферат)

Жак Леб (1859—1924) развивал идею о том, что человеческие организмы представляют собой физико-химические машины, действия которых могут быть об'яснены законами механики. Такой механизм, как ассоциативная память, можно рассматривать, как основу явлений сознания. Память не есть локализованный процесс, совершающийся в мозгу, хотя Леб и считает, что полушария мозга необходимы для ассоциативной памяти. Нервная ткань есть только ткань проводящая.

Так называемая инстинктивная деятельность представляет собой автоматическую ориентировку организма в силовом поле, в направлении к центру или от центра силы. Эти ориентировочные движения, или тропизмы, зависят от неодинаковых химических изменений в симметричных органах чувств, ведущих к неодинаковому направлению в симметричных мышцах; отсюда, повороты и „вынужденные" движения. Такого рода тропизменные реакции на свет, химические раздражители и силу тяжести подчиняются количественным законам.

Целесообразные приспособления, несомненно, об'ясняются наличием у животного наследственной пластичности, благодаря которой животное может существовать и в необычных для него условиях.

Изучение антагонистических действий солей привело Леба к об'яснению возбудимости и сократимости как явлений, вызываемых ионами Na, Ca, K и Mg; стимуляция заключается, повидимому, в замещении ионов Na или K ионами Ca, или наоборот, в той смеси коллоидов, из которых состоит мышца или нерв.

**Пальмер (Palmer).**

# IS THORNDIKIANISM THE LAST WORD IN THE PSYCHOLOGY OF LEARNING?*

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HSIAO HUNG HSIAO

Among the outstanding contributions in the field of psychology, Thorndikianism undoubtedly deserves a place and a very high one. This no one will refuse to admit, yet, with all its merits, this system of thought is loaded with weaknesses. Many of them have already been revealed by Cason,[1] Köhler,[2] Koffka,[3] and others.[4] The laws of learning have been thoroughly criticized; the S-R formula and the trial and error theory have been bitterly attacked; and the fact that such factors as emotion and character have failed to receive due attention in this psychology has been repeatedly pointed out. This looks as if very little remained for the writer to say, if he does not wish merely to reiterate the views of the previous critics, but, in fact, there are still a few vulnerable points left for another attack. Chief among them are the following.

## THE DOCTRINE OF SATISFACTION

In Thorndike's doctrine of satisfaction, there are at least two obscure points; namely, the nature of satisfaction and the mechanism by which the bond is influenced by it.

Satisfaction is defined by Thorndike in his *Educational Psychology* as the conducting of the neural unit that is ready to conduct[5] and in his *Animal Intelligence* as the excitation of peripheral sense-organs.[6]

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[1]Cason, H. Criticisms of the laws of exercise and effect. *Psychol. Rev.*, 1924, **31**, 397–417.

[2]Köhler, W. Mentality of apes. New York: Harcourt, Brace, 1926.

[3]Koffka, K. Growth of the mind. New York: Harcourt, Brace, 1924.

[4]Thurstone, L. L. The nature of intelligence. New York: Harcourt, Brace, 1924.

[5]Thorndike, E. L. Educational psychology, I. New York: Teachers College, Columbia Univ., 1913. See p. 128.

[6]Thorndike, E. L. Animal intelligence. New York: Macmillan, 1911. See p. 148.

In the former definition, satisfaction is conceived as a neural condition, while, in the latter, as a sensory condition. Both definitions contain a partial truth, and it is the partialness of the truth that renders them confusing and misleading, as we shall see later.[7]

With regard to the mechanism by which the bond is influenced by satisfaction, Thorndike asserts that satisfaction functions as a "stamping-in" agent.[8] But as to how and why the bond is stamped in by satisfaction nothing has been said.

First of all, let us see if the definitions of satisfaction will fit in with the "stamping-in" theory. Take for instance the cat in the puzzle box. According to Thorndike's neural conception of satisfaction, all the animal's chance movements ought to be regarded as due to the readiness of their conduction units; for otherwise satisfaction would result from the inhibition of such movements, which is, of course, contrary to the facts. From this it follows that all these chance movements are satisfying. But, if such be the case, why is it that satisfaction stamps out most of them instead of stamping them in?[9] Moreover, considering the logical implications of the conception, it would be natural to suppose that the order of the appearance of the chance movements is determined by the degree of their neural readiness. Then why is it that the very last one of them, whose neural readiness is the least and therefore whose satisfyingness is the least, is stamped in; whereas the others, which are more satisfying due to the greater readiness of their neural mechanisms, are stamped out? A further logical consequence of this concept would be to obviate the necessity of the bond's being stamped in by any satisfaction other than the mere exercise of the bond, which is supposed to be satisfying in itself. In other words, the satisfaction arising out of the consummatory act, such as the eating of food, should bear no essential relationship to the bond that is stamped in, but this would be in direct contradiction to the law of effect, which is so strongly emphasized by Thorndike himself.

Turn now to his other or sensory conception of satisfaction. There are at present no facts which suggest much relationship between this conception and the "stamping-in" theory. In the first place, Thorndike has never proved by experiments, in which covered food is to serve as the *only* incentive, that such peripheral excitation is an effective "stamping-in" agent. In the second place, he seems to have

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[7]There is, indeed, a third definition of satisfaction, viz., "By a satisfying state of affairs is meant one which the animal does nothing to avoid." (*Ibid.*, p. 245.) Since, however, this definition is a mere tautology, it may be dismissed from our discussion.

[8]Thorndike, E. L. Educational psychology, I. See p. 172.

[9]Thorndike, E. L. Animal intelligence. See p. 147.

taken for granted that animals are always fed after learning experiments. But, in many cases, no such purely sensory excitation as receiving food needs to occur in order to produce learning.

The discussion has thus led to a crucial point, i.e., either the definitions of satisfaction are inadequate, or the "stamping-in" theory is wrong, or both are misleading.

Let us first examine the definitions of satisfaction. They are, of course, meant to be two aspects of one and the same thing; but the trouble is that, even if they were combined into one definition, a large part of the truth would still remain excluded. As a matter of fact, with the exception of a small number of activities which are satisfying in themselves, the mere conducting of the neural unit that is ready to conduct is not satisfying. *In order to be satisfying, such conduction has to result in a need's being fulfilled.* For instance, to a hungry baby,[10] merely to suck is not satisfying. Satisfaction results only from the fact that there is milk to swallow. As to the aspect of sensory excitation, whether or not it is satisfying depends upon the kind of need. In case of the need that is utilized by Thorndike in his animal studies, mere peripheral excitation is certainly *not* satisfying; for otherwise Thorndike, in his learning experiments, would have let his animals *see* the food only instead of actually feeding them.

In regard to the "stamping-in" theory, attention is to be called to the fact that, if it were true that successful responses are retained merely owing to the "stamping-in" effects of satisfaction, then the tendencies of the "stamped-in" responses ought to be always in the same direction. In other words, *all responses of whatever kind would tend to increase steadily in readiness with the number of times such effects are supposed to take place. But the reverse is shown by Thorndike's own experimental evidence.* For instance, such responses of the cat as clawings, pullings, scratchings, etc., tend to persist even in changed situations, whereas the response of licking or scratching herself tends to disappear even in the situations that have undergone no change at all. This divergence of tendencies Thorndike confesses his inability to explain. It is undoubtedly his "stamping-in" theory that blinds him to other possibilities of explanation.

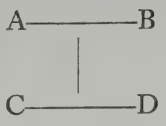
Another aspect of the "stamping-in" theory is that satisfaction stamps in a bond because of its temporal closeness to it.[11] Let us see if this can really serve as a principle of explanation.

We may denote the box situation by A and the successful re-

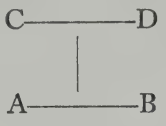
[10]In this instance we are talking about the need of hunger, but of course the baby has other needs as well.

[11]Thorndike, E. L. Educational psychology, I. See p. 172.

sponse to it by B, the food situation by C, and the response of eating by D. Then we have




Here the bond between C and D and that between A and B are temporally close to each other, and the former does strengthen the latter. But suppose their temporal closeness remains the same, while their temporal relationship is reversed as




It requires no experimental evidence to prove that, in this case the bond between A and B will not be strengthened by that between C and D. This fact throws sufficient doubt on the validity of the principle of temporal contiguity. To this criticism the reply would naturally be: Satisfaction precedes the bond in the one case, but follows it in the other, and that is the cause of the difference. But the real question involved in this criticism is: Why is it that satisfaction stamps in a bond only when following it? This is a question unanswerable in terms of any of Thorndike's laws.

Moreover, in view of the recent discoveries, we have to say that *temporal contiguity even in the sense of closely following the bond to be fixated, is not such an important factor as Thorndike supposes.* For instance, C. J. Warden[12] has found that a five-minute interval of delay between the act to be fixated and the feeding response had no measurable effect upon the rate of fixation. How would Thorndike explain this fact? Is the law of effect thereby invalidated? These and the other questions that have arisen in this discussion will be considered in a later section of the paper.

## The Concept of Situation

Another concept I wish to consider is that of situation. This word is primarily intended by Thorndike to serve as a substitute for the term "stimulus." This is clearly seen in the following statements. "Any fact of intellect, character, or skill means a tendency to respond in a certain way to a certain situation—involves a situation or state of affairs influencing the man, a response or state of affairs in the man,

[12]Warden, C. J., & Haas, E. L. The effect of short intervals of delay in feeding upon speed of maze learning. *J. Comp. Psychol.,* 1927, **7**, 107–116.

and a connection or bond whereby the latter is the result of the former."[13] "Each situation will be stated as just this state of affairs in nature: the response will be stated as just this event in the man; and the bond will be stated as just this set of habits or just that arrangement and condition of the man's neurones by which the event in the man is brought to pass when that state of affairs is present in nature."[14] "These constitute an enormous fund of connections or bonds of varying degrees of directness and strength between situations furnished by physical forces, plants, animals, and the behavior of other men and the responses of which the human creature is capable."[15] Besides these, numerous other statements might be cited to illustrate Thorndike's externalistic conception of situation. Moreover, it is owing to the very fact that he regards situation as exclusively external that such a factor as set or attitude is introduced to explain the variation of responses. The most striking expression of this externalistic inclination is the substitution of time as a situation for the physiological conditions of the organism in the statement, "To the situation, a certain period of life, and, in the male, a certain interval since the last discharge of spermatozoa, the response is a restlessness, etc."[16] So far, the statements sound as if Thorndike's externalistic conception were pure and simple.

But, now, let us read the statement, "The bonds lead not only from external situations—facts outside the man—to responses in him, and from situations in him to acts by which he changes outside nature, but also from one condition or fact or event in him to another and so on in long series."[17] Here the internal factors of the organism are brought in as situations. This would be perfectly justifiable if not for the fact that it is inconsistent with his original definition of situation. Moreover, from this quotation, we can easily see that the situation is here regarded either as exclusively external or exclusively internal. *But the fact is that, as a rule, the situation at any one moment is neither exclusively external nor exclusively internal.* For instance, the same food served at the same place would have a different meaning for the same organism according as it is in a state of satiety or hunger. Again, the same state of the organism would be felt differently in the presence and in the absence of food. To detach the internal aspects of the situation from its external ones, or the reverse, would inevitably invalidate the fundamental premise so much emphasized by Thorndike that the same situation always produces the same response.

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[13]Thorndike, E. L. Educational psychology, I. See p. 1.
[14]*Ibid.*, I, 10.
[15]*Ibid.*, II, 1.
[16]*Ibid.*, I, 98.
[17]*Ibid.*, II, 54.

## The Bond Theory

According to Thorndike, it is to the elements of a situation that responses are attached.[18] Therefore, "transfer occurs only where identical elements are concerned in the influencing and the influenced functions."[19] This exclusive emphasis upon the elemental aspect of the situation is in direct defiance of facts. The clearest instance is furnished by the behavior of Köhler's apes. In one of his experiments, they were trained to pick their food from the darker of two gray containers. When a third container, darker than that which had been responded to, was introduced into the situation, this was chosen instead of the identical one. This experiment alone is sufficient to show that it is not always to the elements of a situation that responses are attached.[20]

In order to force facts to suit his theory, Thorndike is often compelled to use such terms as "abstract or subtle elements" to denote beauty, truth, importance, etc.[21] But, in fact, these abstractions usually indicate the relations of elements rather than the elements themselves. Therefore, here, the word "element" is not used in its ordinary sense; it means *a relation or total characteristic of an object or affair instead of its component.* This shift from one meaning to another can be regarded only as either of the two things: lack of precision in thought, or unwillingness to recognize facts as facts, if they do not suit preconceived notions.

Moreover, in discussing the improvement of such functions as knowledge of grammar and logic or solving problems in arithmetic or originals in geometry or ability to solve intellectual puzzles, Thorndike says, "The share of changed bonds in the improvement is seen to increase when bonds with elements, aspects and relations as well as with gross total situations themselves are considered."[22] Here the importance of relations is explicitly recognized, but this admission is unfortunate in the fact that it directly contradicts his elemental premise.

Another aspect of the bond theory is the additive nature of the bond.[23] In the eyes of Thorndike, changes in behavior are merely due to the addition or subtraction of bonds or satisfyingnesses. To examine the validity of this view, let us consider the following simple illustration. It is an indisputable fact that man responds to a

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[18] *Ibid.*, II, 32.

[19] *Ibid.*, II, 399.

[20] This criticism has already been made by Gestalt psychologists, but it is brought in here to be expanded.

[21] Thorndike, E. L. Educational psychology, II. See p. 419.

[22] *Ibid.*, II, 189.

[23] *Ibid.*, I, 10, and II, 186.

tiger by fleeing; to a cage by indifference; and to the situation composed of a tiger and a cage, i.e., the situation of a tiger's being in a cage, by acts expressive of curiosity. According to the additive theory, man's response to the last situation should be either that of fleeing or that of indifference or a combination of the two! Why is it then that an entirely different response appears? To account for this, one might say that a tiger's being in a cage is an unusual situation, and therefore a response expressive of curiosity is called forth. But the fact is that we are curious, not about the tiger's being in a cage, but about the tiger itself.

A parallel but less obvious illustration is furnished by Thorndike himself, which may be quoted here. "First, the apparently same situations may really be different. Thus, the sight of a dog to an infant in its mother's arms is not the same situation as the sight of a dog to an infant alone on the doorstep. Being held in its mother's arms is a part of the situation that may account for the response of mild curiosity in the former case, and fear in the latter."[24] This explanation again shows the straining tendency mentioned above, for being held in its mother's arms indicates a relation rather than an element. Moreover, this relation alone does not satisfactorily explain the response of curiosity.

There is one more aspect of the bond theory that remains to be considered. According to Thorndike, it is always between a situation and a response that bonds are formed. But, actually, this is not the case. For instance, we are told that it is the function of the mental set to determine other responses. Having such a function, the mental set ought to be regarded as a situation according to Thorndike's theory; but it is conceived as a response.[25] Here emerge two important facts. First, *bonds are not necessarily formed between situations and responses, as Thorndike supposes, and, second, with the exception of reflexes, instead of being directly attached to situations, responses are connected with mental sets or perceptions of situations.* Both these facts are themselves refutations of Thorndike's bond theory.

Futhermore, it is hard to visualize the existence of bonds between situations and responses except in terms of neurones. Once we think in such terms, we are compelled to conclude that situations become our situations only when we have perceived them; and that, being perceived, they are no longer mere situations. They are then situations plus responses, for perceiving is responding. The latter fact is even recognized by Thorndike himself.[26] In many cases, only

[24] *Ibid.*, I, 7.
[25] *Ibid.*, II, 420.
[26] *Ibid.*, I, 43.

such responses are needed; although, in others, other responses are required. Therefore perception is at least an incipient response. Viewed in this light, bonds exist then, not between situations and responses, but between perceptual responses and those of other kinds.

## Suggestions

Thus far we have been dealing with the inadequacies and inconsistencies that exist in the Thorndikian system of thought. It is our task now to attempt to solve some of the difficulties that have appeared in that system. The first point must be to stress the factor of need. This factor is essential in any and every effective process of learning. Upon its importance the following hypothesis is based.

It is reasonable to suppose that the need is felt only when there is stimulation in the neurones that are directly concerned with it. Such a stimulation, once it starts, tends to irradiate the other neurones that are either directly or indirectly connected with the satisfaction of the need. Take Thorndike's cat for instance. It is her need of getting out or getting food that sets in action the mechanisms concerned in her clawings, bitings, scratchings, etc. All these so-called chance movements of the animal are but irradiations of her need.

In the trial and error process of learning, it is the last of these irradiations that is satisfying, because it is the last irradiation that fulfils the need, and thus relieves the tension which accompanies the need and causes irradiations. Viewed in this light, satisfaction is not anything extraneous to the successful response but is essentially connected with it. This, I believe, is the correct conception of satisfaction.

As to the effect of satisfaction upon the successful response, we may say that, in a given situation, once there occurs an irradiation by which the consummatory response (e.g., that of eating) is made possible, that irradiation will tend to become linked with the irradiation that underlies the consummatory response: for, whenever the same need is to be satisfied in the same situation, there has to be that same irradiation; and, owing to this necessity, that particular irradiation occurs not merely as a forerunner of the consummatory response that is made in that particular situation, but also in connection with both that particular need and that particular situation. Now, as the consummatory response is the expression of the primary impulse of the need, it is natural that, if the animal is later placed in the same situation when it has the same need, the irradiation that has been linked up with the consummatory act will be more excitable than those irradiations that have failed to be so connected. In other words, upon the return of the same need in the same situation, the nerve course concerned in the act that has been connected with

the end situation (e.g., the food situation in the case of Thorndike's cat) is more excitable than all those nerve courses concerned in the unsuccessful acts, and therefore more ions pass from the nerve endings of the neurones concerned with the need into the cells of the neurones concerned in this irradiation. This makes it possible for a smooth path to be formed between these two groups of neurones, and the successful act is thereby retained.

As to the psychic aspect of the process, I shall do nothing more than merely mention the desirability of assuming some representation of the end situation as playing a part, in later trials,[27] in the process of irradiation that is connected with the end situation. This assumption is particularly desirable in cases where many steps are involved in a successful performance.

To account for the diverging tendencies of the learned responses of Thorndike's cat, we may assume that, with reference to a particular need, there are natural and unnatural irradiations. Its natural irradiations are those that occur where the nerve courses, by virtue of nature, offer little resistance when the stimulation from the center of that need radiates through them. The bitings, clawings, and scratchings of Thorndike's cat were more or less natural irradiations of the need that was then created by the situation. On the other hand, the unnatural irradiations of a need are those that occur where the nerve courses, by virtue of nature, offer great resistance when the stimulation from the center of that need is forced to radiate through them. The cat's response of licking or scratching herself in connection with hunger is exactly such an irradiation. Between these two extremes, there are of course many degrees of naturalness and unnaturalness of irradiations. It would not be illogical to suppose that the more unnatural an irradiation with reference to a need, the more its nerve course tends to be disconnected with that need, and vice versa. The latter point is too obvious to need any proof. As to the former, it is strongly suggested by the fact that, as a rule, conditioned reflexes require many repetitions for their formation and yet easily die out.

So much for satisfaction, and let us now turn to the question of situation. I have already emphasized the fact that our situation at any one moment is neither exclusively external nor exclusively internal. This, of course, does not mean that purely external situations do not exist, but this does mean that situations as such are not situations for us until they have been somehow placed in our own experiences. In other words, with the exception of reflexes, the

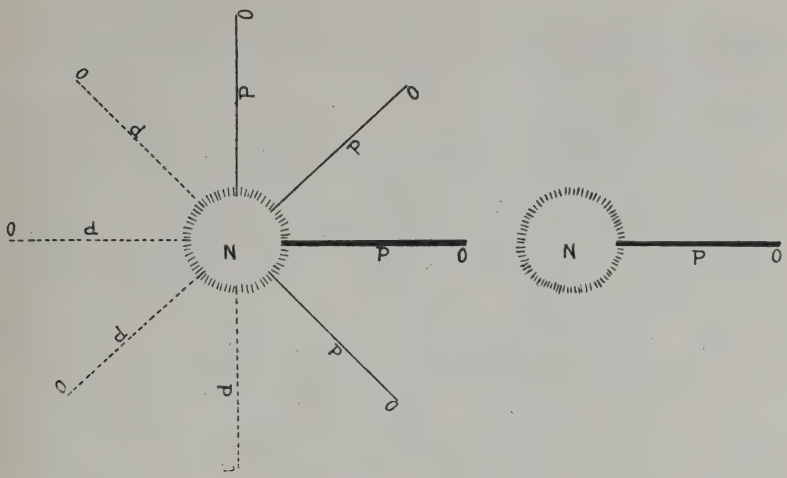
[27] Tolman, E. C. A behaviorist's definition of consciousness. *Psychol. Rev.*, 1927, **34**, 433–439.

situation we respond to by acts other than perceiving is always a perceived situation. As to how the situation is perceived at any one moment, this depends upon the need of the organism at that moment. Therefore, perception, like other responses, is an irradiation, or rather a resultant of irradiations of the need. Put in another way, it is the psychical aspect of the irradiations in the nerve courses that lead from the center of the need. Viewed in this light, perception is the final irradiation, or rather final product of irradiations of the need, when it is the only response required for the situation, but it represents merely a transitional stage in these irradiations, if other responses are required. Very often the latter is the case. Some might think that, in the case of habitual responses, perception is unessential, but this is not true. Take typewriting for instance. It is a truism that, however skilful she may be, the typist has to tell what kind of typewriter she is to use before putting her fingers on it. In fact, the only difference between a habitual response and a new one, in respect to the attention needed for the situation, is that it is maximum in the case of the latter but minimum in the case of the former.

With regard to the bond theory, its fallacies have already been pointed out. Let us now call attention to the bearings of the irradiation hypothesis upon this theory.

From what has been discussed in the preceding sections of the paper, it is safe to conclude that no bond exists between the external situation and the overt response, for the same external situation does not always produce the same overt response; that no bond exists between the external situation and the perception of it, for the same external situation does not always call forth the same perception; and that it is only between the perception and the overt response that a bond may be said to exist. But even this last-mentioned bond is not a bond by itself, for both the perception and the overt response start from a need—a common need—and it is through this common need that such a bond comes into existence. In other words, it is the need that connects the perception and the overt response. Therefore, it seems more correct to regard them both as irradiations of the need, although they represent different stages of these irradiations.

To make clear the hypothesis set forth above, we may represent the trial and error process of learning and its product by the following two diagrams. In Figure 1, $N$ stands for the center of a need, $P$ for a perception of a situation, and $O$ for a chance overt response to the situation. The continuous lines represent natural irradiations of the need, whereas the dotted lines represent its unnatural irradiations. The heavy lines represent the successful response, the heaviness of the line indicating the intensity of the irradiation in the nerve




FIGURES 1 AND 2

course. Figure 2 represents the final product of learning. Here the perception and the overt response are fixed. In both diagrams, we can easily see that the perception and the overt response merely represent two stages in the irradiations of the need, but it should be understood that these lines often represent a series of resultants of irradiations rather than a series of single ones.

## Summary

In this critical review of the Thorndikian psychology, three fundamental notions have been considered; namely, the doctrine of satisfaction, the concept of situation, and the bond theory. The conclusions arrived at in the above discussion may be summarized as follows:

1) *The Doctrine of Satisfaction.* We have considered this doctrine in two aspects, viz., the concept of satisfaction and the "stamping-in" theory.

*a*) The concept of satisfaction. Satisfaction is defined by Thorndike sometimes as neural conduction and sometimes as sensory excitation.

(1) It has been shown that both these definitions do not fit in even with the "stamping-in" theory.

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[28]Tolman, E. C. Habit formation and higher mental processes in animals. *Psychol. Bull.,* 1928, **24**, 49–51.

(2) They both are inadequate, for, in the major portion of human experiences, neither mere neural conduction nor mere sensory excitement is satisfying. In order to be satisfying, they both have to produce desired results.

*b*) The "stamping-in" theory. According to this theory, bonds are acquired owing to the "stamping-in" effects of satisfaction.

(1) This theory does not explain the diverging tendencies of the learned responses of Thorndike's cat. The results of his experiments show that such responses of the cat as clawings, pullings, scratchings, etc., tend to persist even in changed situations, whereas the response of licking or scratching herself tends to disappear even in the situations that have undergone no change at all.

(2) The assumed effect of the temporal closeness of satisfaction upon the bond to be fixated is shown to have its limitations.

(3) Recent discoveries tend to diminish the exaggerated importance of the temporal contiguity of satisfaction with the bond to be fixated, even in the sense of closely following it.

2) *The Concept of Situation*

*a*) Thorndike's conception of the situation is shown to be primarily externalistic.

*b*) This conception is not consistently maintained throughout his exposition, for sometimes he is compelled to bring in internal factors. However, he tends to regard situations either as exclusively external or as exclusively internal.

*c*) Attention is called to the fact that the situation at any one moment is neither exclusively external nor exclusively internal.

3) *The Bond Theory*

*a*) According to Thorndike, it is to the elements of a situation that responses are attached. However, he has difficulties in proving that this is always the case; and, for this reason, inconsistencies in this respect are found in his writings.

*b*) The fallacy of the additive nature of the bond has also been shown.

*c*) Bonds are not between situations and responses, as Thorndike supposes; but, with the exception of reflexes, responses are connected with perceptions of situations instead of being directly attached to situations themselves.

*d*) Thinking in terms of neurones, it is easy to see that situations become our situations only when we have perceived them, and that, once being perceived, they are situations plus responses, for perceiving is responding. Therefore, bonds, if they can be conceived at all, exist not between situations and responses, but between perceptual responses and responses of other kinds.

4) *Suggestions*

*a*) To answer the problems arising in the discussion, a hypothesis is proposed, which regards the need as the center of behavior irradiations, and the neural nature of such irradiations as the basis upon which the selection of responses and their later tendencies are determined. Viewed in this light, satisfaction is not anything extraneous to the successful response, but is essentially connected with it. As to the effect of satisfaction, it does not function as a "stamping-in" agent, but as an intensifier of the nerve course that leads from the center of a need.

*b*) With the exception of reflexes, the situation we respond to by acts other than perceiving is always a perceived situation. Perception, like other responses, is an irradiation of a need. Such an irradiation is final when serving as the only response needed, but transitional when other responses are required.

*c*) The bond exists, not between the external situation and the overt response, nor between the external situation and the perception of it, but between the perception of the situation and the overt response to it. Since both the perception and the overt response start from a common need, it will be more correct to regard them as irradiations of the need, although they represent different stages in these irradiations.

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## LE "THORNDIKIANISME" EST-IL LE DERNIER MOT DE LA PSYCHOLOGIE DE L'APPRENTISSAGE?

(Résumé)

A l'égard de la doctrine de la satisfaction, ni la conduction neurale ni l'excitation sensoriale *per se* ne produisent la satisfaction à moins que les résultats désirés ne soient ainsi produits. Un autre aspect de cette doctrine est la théorie de "stamping-in" (impression). Puisque cette théorie exige un accroissement régulier de la promptitude des réponses avec la fréquence des effets "stamping-in" de la satisfaction, elle n'explique pas les tendances divergentes des réponses du chat de Thorndike.

On a montré que la situation à un moment donné n'est ni exclusivement externe ni exclusivement interne selon la croyance de Thorndike.

Dans la discussion de la théorie des liens ("bond theory"), on a donné des preuves pour montrer la fausseté des vues suivantes:

1) Les réponses sont attachées aux éléments d'une situation.

2) Les changements du comportement sont dus simplement à l'addition ou à la soustraction des liens ou choses satisfaisantes.

3) Les réponses sont toujours attachées aux situations.

A l'exception des réflexes, la situation à laquelle on répond par des actes autres que ceux de la perception est toujours une situation perçue, et le

lien existe seulement entre la perception de la situation et la réponse ouverte. Puisque la perception et la réponse ouverte commencent par un même besoin, il sera plus correct de regarder toutes les deux comme des irradiations du besoin, bien qu'elles représentent des étapes différentes de ces irradiations.

HSIAO

## IST DER THORNDIKIANISMUS DIE ENDGÜLTIGE LÖSUNG (THE LAST WORD) DER PSYCHOLOGIE DES LERNENS?

(Referat)

Was die Theorie der Befriedigung anbetrifft, so können weder Nervenleitungen noch sensorische Erregung an und für sich Befriedigung erzeugen, wenn nicht eben dadurch das erwünschte Resultat bewirkt wird. Ein anderer Aspect dieser Lehre ist die Theorie des "Einprägens." Da diese Theorie stetig zunehmende Fertigkeit im Beantworten voraussetzt, je nach der Häufigkeit der "Einprägungswirkungen" der Befriedigung, so mangelt sie jeder Erklärung der so verschiedenen Reaktionen der Katze Thorndikes.

Man hat darauf hingewiesen dass die Situation zu keiner Zeit ausschliesslich äusserlich noch ausschliesslich innerlich ist, wie Thorndike glaubt.

In der Erörterung der (bond) Verbindungstheorie hat man Beweise für die Unhaltbarkeit der folgenden Ansichten beigebracht:

1) Die Reaktionen hängen von den Unständen der Situation ab.
2) Veränderungen im Verhalten werden einfach durch das Hinzufügen oder Entfernen der Verbindung oder der Befriedigungen veranlasst.
3) Reaktionen sind immer mit Situationen verknüpft.

Mit der Ausnahme von Reflexen, ist die Situation, auf die wir tätig reagiren, immer eine wahrgenomme Situation, und die Verbindung besteht nur zwischen der Wahrnehmung der Situation und der offenbaren Reaktion. Da nun die Wahrnehmung sowohl wie die Reaktion aus demselben Bedürfnis entspringen, so ist es wohl richtiger beide als Ausflüsse des Bedürfnisses zu betrachten, obwohl sie verschiedne Stadien dieser Ausstrahlungen darstellen.

HSIAO

## ЯВЛЯЕТСЯ ЛИ ТЕОРИЯ ТОРНДАЙКА ПОСЛЕДНИМ СЛОВОМ В ПСИХОЛОГИИ ОБУЧЕНИЯ?

(Реферат).

Ни нервная проводимость, ни сенсорное возбуждение сами по себе не достаточны для выставленной Торндайком теории удовлетворения; необходимо еще, чтобы при этом достигались желательные результаты. Другой аспект этого учения дает теория „запечатления"; так как эта теория имеет в виду постоянное увеличение готовности к реакции с частым повторением и „запечатлением" удовлетворения, она не объясняет еще отклонений, получаемых в реакции кошки Торндайка.

В анализе результатов следует исходить из того, что ситуация в любой момент не является исключительно внутренней или внешней, как полагает Торндайк.

Критикуя теорию связей, автор указывает на необоснованность следующих положений:

1) что реакции относятся только к отдельным моментам ситуации,

2) что изменения в поведении являются результатом увеличения или уменьшения связей или удовлетворенности,

3) что реакции связаны только с ситуацией.

За исключением случая рефлексов, ситуация, на которую мы реагируем, всегда является воспринятой ситуацией, и связь существует только между восприятием ситуации и открытой реакцией на нее. Так как восприятие и открытая реакция проистекают из общей необходимости, правильнее рассматривать их, как результанту этой необходимости в различных ее проявлениях.

**Сяо (Hsiao).**

# NOTES

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## A TRIDIMENSIONAL MAZE

There are three fundamental types of mazes suitable for use with rats: (1) the bidimensional spatial maze, which is the customary one employed; (2) the temporal maze described by Hunter (1 and 2); and (3) the tridimensional maze described in the present paper. Numerous variations are possible within each type, and various combinations of the three types can be made. So far no one has published work upon a combination of types 1 and 2, a combination the investigation of which is under way in this laboratory.

Before describing the new maze it will be well to consider briefly certain differences between mazes of types 1 and 2. The bidimensional spatial maze differs from the temporal maze in several very important particulars. In the spatial maze as the subject runs from entrance to exit, he is not only entering new units of space but he is extending the reaction through new units of time. In the new spatial units he will encounter new stimuli from the maze and also probably new stimuli from the environment of the maze. Furthermore, theoretically, a directional tendency may be built up which to some degree regulates the correct run from entrance to exit.

In the temporal maze the subject runs through the same unit of space in successive units of time, the actual pathway being varied by opening and closing doors. After the subject has once passed completely through a unit of the temporal maze, there are no new exteroceptive cues, either within or without the maze, for him to encounter. The temporal maze therefore enables us to control all exteroceptive stimuli by rendering them uniform from unit to unit of the maze. Furthermore, a directional tendency could hardly appear in the temporal maze habit as it may very well appear in a bidimensional spatial maze habit. Another important difference between a temporal maze and a spatial maze of type 1 may be indicated as follows: The temporal maze has at least one alley which is common to the various responses that make up the total run of the maze. The animal passes through this alley and goes around, let us say, the right side of the maze whereupon he again reaches the central alley. He must now go again through this alley and turn left. This again brings him to the central alley, and the behavior continues in this manner. The importance of this common alley seems to lie in the fact that, since it is common to at least two different responses, the differential cues for these responses cannot lie within it. (See Hunter, 2.) The stimuli which the subject encounters in this alley can have only a releasing function.

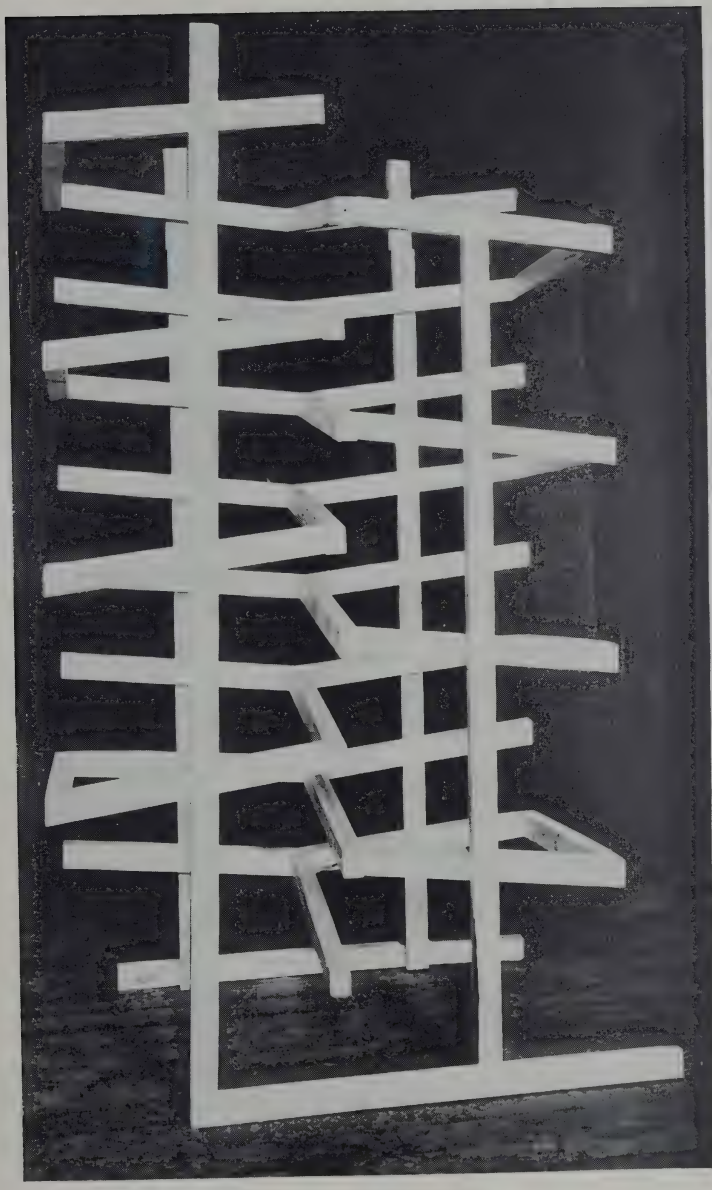

FIGURE 1

FIGURE 2

We now come to the consideration of the tridimensional maze. The figures of the present paper show the details of this new maze in the form in which we have used it. So far our tridimensional mazes have involved either a simple alternation, *right, left, right, left,* as shown in Figure 1, or a series of double alternations, *left, left, right, right,* as shown in Figure 2. These arrangements are, of course, not essential to the plan of tridimensional mazes in general. Again, in the mazes illustrated in Figures 1 and 2, all central alleys lie in a common vertical plane and the true pathway always leads upward at a constant angle. Neither of these characteristics is necessary. A tridimensional maze could readily be built whose true pathway was sometimes ascending and sometimes descending and whose true pathway never involved central alleys placed in a single vertical plane. An investigator, however, does not—at least he should not—construct a maze different from those previously used merely for the sake of having something different. The present plan of the tridimensional maze was adopted for a very specific reason. Rats, even when blind, had shown an ability to master a double alternation, *llrr,* bidimensional spatial maze, the units of which were T-shaped and structurally all alike. Rats, however, had failed to master the double alternation problem either in a temporal maze or in a discrimination box. The problem thus arose of getting a maze which would have some of the characteristics of each of the two types just mentioned. The double alternation tridimensional maze of Figure 2 was our answer to the problem. The results show that this maze can be learned by normal rats and that some rats who can run the maze perfectly do not immediately lose that ability when blinded.

The present form of the tridimensional maze utilized an elevated runway after the model of Miles's (3) very convenient variation of Vincent's (4) sideless maze. (It would be possible of course to construct a tridimensional maze with alleys having sides as in the usual maze.) In Figure 2, as in the temporal maze which rats have failed to learn, each unit is T-shaped. Furthermore, all of the four units are precisely alike in width, depth, and length of straight sections. The units are carefully joined, and the whole is thoroughly sandpapered and kept clean. Each piece of wood of which the units are constructed is 1½ in. wide, 1 7/8 in. deep, and 20 in. long. Each alley rises at the rate of 2 in. in 20 in. The clearance between the top of one alley and the bottom of the alley above it is 6 1/8 in. The uprights which support the maze are nailed directly to the alleys and are placed not less than 7¼ in. from the nearest side of the central alley. A small piece of wood was tacked on the exit of the maze to serve as a food platform. When the rat reached this platform, he was allowed one or two bites of food before being removed from the maze. A light was suspended over the center of the maze; and, in order to eliminate shadows, a piece of white paper was placed on the top of the uprights and beneath the light. This resulted in a mild diffused light all over the apparatus. Because of the sideless character of the maze and the consequent ease with which stimuli from the environment could get access to the subject, the experimenter stood behind a screen and remained absolutely quiet while the rat was on the maze.

As the rat runs through the maze of Figure 1 or 2, he is entering new units of space as well as new units of time. In this respect the maze is similar to an ordinary double alternation spatial maze. (These new spatial units may be made perfectly uniform, and yet, since the second unit, for example, is actually not the first unit, new stimuli may be encountered in it. This factor is controlled in the temporal maze by having only one structural unit through which the animal can run.) The common central

alley of the temporal maze is approximated by placing the central alleys of the tridimensional maze in one vertical plane. This keeps the absolute orientations of the central alleys the same and makes it possible for the food platform to be located directly above the entrance to the maze. In the bidimensional maze it is impossible to construct identical units whose points of choice have the same absolute orientation. If directional tendencies function in maze learning, they must be horizontal in bidimensional mazes and in some degree vertical in the present tridimensional mazes. This latter type of maze radically changes the relationship between maze and environment from the condition which holds with bidimensional mazes. It is impossible with the present maze to eliminate the possibility of differential cues from the environment by rendering them constant from unit to unit as is done in the temporal maze. However, by placing the units in a vertical order, we can insure the same relationship between each point of choice and such environmental stimuli as sound. The only environmental factors which can be effective are those which vary as the rat climbs higher in the maze. Temperature and olfactory differences are the obvious factors which might vary in a vertical section of the ordinary laboratory environment.

A brief statement of certain results will be of interest in the present connection. Of 23 normal, untrained rats who were tested on a maze like that of Figure 2 but having the turns in the order *rrll*, only one rat made 3 correct trials in succession during a total of 86 trials. Each of 4 rats of the 23 made only one correct trial in the 86 trials. When the rat who mastered the problem was blinded, he could still occasionally run the maze correctly; but at the time of the present writing he seems to be losing his mastery, although his health is apparently perfect and although neither the apparatus nor the method has been changed.

Eleven normal, untrained rats were trained in an exactly similar environment, except that the illumination was slightly higher, and by the same method on a maze in which the turns at the point of choice were *llrr*. Six of these rats mastered the maze within 30 trials. The others were still imperfect at the end of 38 trials. At the close of 38 trials, only 3 rats had never made a perfect trial at some time or other during the training. Four of the 6 rats who had mastered the maze were blinded, and all showed an ability, regularly or occasionally, to make perfect records. The present note is not the place for a consideration of the probable reasons for the different performances of these two groups of subjects nor for the presentation of the extended experimentation which has been done on various aspects of the tridimensional maze. A full account of the experimental work will appear later in the *Journal of Genetic Psychology*.

## References

1. Hunter, W. S. The temporal maze and kinaesthetic sensory processes in the white rat. *Psychobiol.*, 1920, **2**, 1-17.
2. ———. The behavior of raccoons in a double alternation temporal maze. *J. Genet. Psychol.*, 1928, **35**, 374-388.
3. Miles, W. R. The narrow path elevated maze for studying rats. *Proc. Soc. Exper. Biol. & Med.*, 1927, **24**, 454-6.
4. Vincent, S. B. The function of the vibrissae in the behavior of the white rat. *Behav. Monog.*, 1913, **1**, No. 5.

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## THE STROBOPHOTOGRAPH: A DEVICE FOR MEASURING PITCH

Psychophysical studies of melody in speech, vocal movements in emotional expression, voice patterns and unusual intervals in folk music, and similar problems involving the measurement of frequency of vibration, have been slowed up by the expense and labor of measuring the sound waves after they have been photographed. The strobophotograph has been satisfactorily used in the Iowa laboratory as a means of photographing and measuring the sound waves in one process. Phonophotographic studies of vocal and instrumental music may now proceed at one-fifth the expense of photography, and one-fiftieth the labor of measurement.

The device here described is the one which best fits a particular group of problems, and is to be considered as merely exemplary of a class of instruments. The present model of the strobophotograph is adapted to measuring sound waves either from the voice directly, over the radio, or from phonograph records. Simpler models are now being constructed for special, practical purposes.

*The principle.* The device uses a new principle for the measurement of frequency of vibration. There is a synchronizing of flashes of light with moving holes as in stroboscopy, with the difference that the effect is photographed rather than seen.

In stroboscopic observation, as on the Seashore tonoscope,[1] a row of dots on a drum appears stationary when there are as many flashes of light as there are holes passing a given point. For example, a row of 125 equally spaced holes revolving once per second would be seen when there are 125 light flashes per second. However, in order that the row of holes seen standing still should be photographed as dots by the new principle, it is necessary that the film, like the eye, be stationary.

For photography with a moving film, in order to measure successive changes of frequency of vibration, the row observed is not the row photographed. On the Seashore tonoscope, the rows adjacent to the one apparently stationary appear to be moving. The rows with a few more holes per second than there are flashes of light move upward and those with a few less, downward. The row which photographs as dots in moving photography, shown in Figure 2, is the one which to the eye would move at the same speed and direction as the film is moving. The dots in Figure 2 may be connected with lines, as in any graphic procedure.

*Apparatus.* In Figure 3 a horizontally revolving drum *A*, 70 cm. in diameter, has a pressboard disc *B*, 91 cm. in diameter, projecting from its outer edge. In this projecting disc *B*, there are 66 rows of equidistant holes. These holes have a diameter of approximately .6 mm., and vary in number from 244 on the inner circumference to 517 at the outside, in steps equivalent to the nearest integer representing one-tenth of a tone of the even tempered scale *A*, 435 dv. The drum *A* is revolved by a motor which is driven at a relatively high rate so that the drum revolves fairly regularly.

A smaller drum *C* drives the photographic paper in a ratio of 1: 6 revolutions with the drum *A*, by use of the bevel gears *D* and *E*, when the steel plate *F*, attached to the axle of the gear *D*, is engaged with the perforated steel plate *G*, attached to the axle of the drum *C*, by means of the pin *H*. This arrangement permits the stopping and starting of the photographic paper without adjustment of the motor. The perforations in the plate *G* are in line with the pin H, and there are a sufficient number so as

[1]Seashore, C. E. The tonoscope. University of Iowa Studies in Psychology, No. VI. *Psychol. Monog.*, 1914, **16**, (no. 69). See pp. 1-12.

FIGURE 1

PHOTOGRAPH OF THE PRESENT MODEL OF THE STROBOPHOTOGRAPH

to start the drum *C* almost immediately. A small leather and spring brake *I* is necessary to keep the drum *C* stationary when the pin *H* is out because of the necessary friction between the plates *F* and *G*.

A beam of light *J* is projected from a Dorsey phonelescope[2] *K* (a diaphragm with mirror attached so that mirror rocks with the motion of the diaphragm), coming from an automobile bulb in the light housing *L*. A cylindrical lens *M* focuses the light on the disc B. The phonelescope *K* is vibrated either by sounds coming from the horn *N* or from the phonograph records on the turntable *O*, picked up by the tone arm *P*. The revolving pressboard disc *B* is held in one position by the guide *Q*, at the point of

[2] *J. Opt. Soc. Amer.*, 1922, **6**, 279.

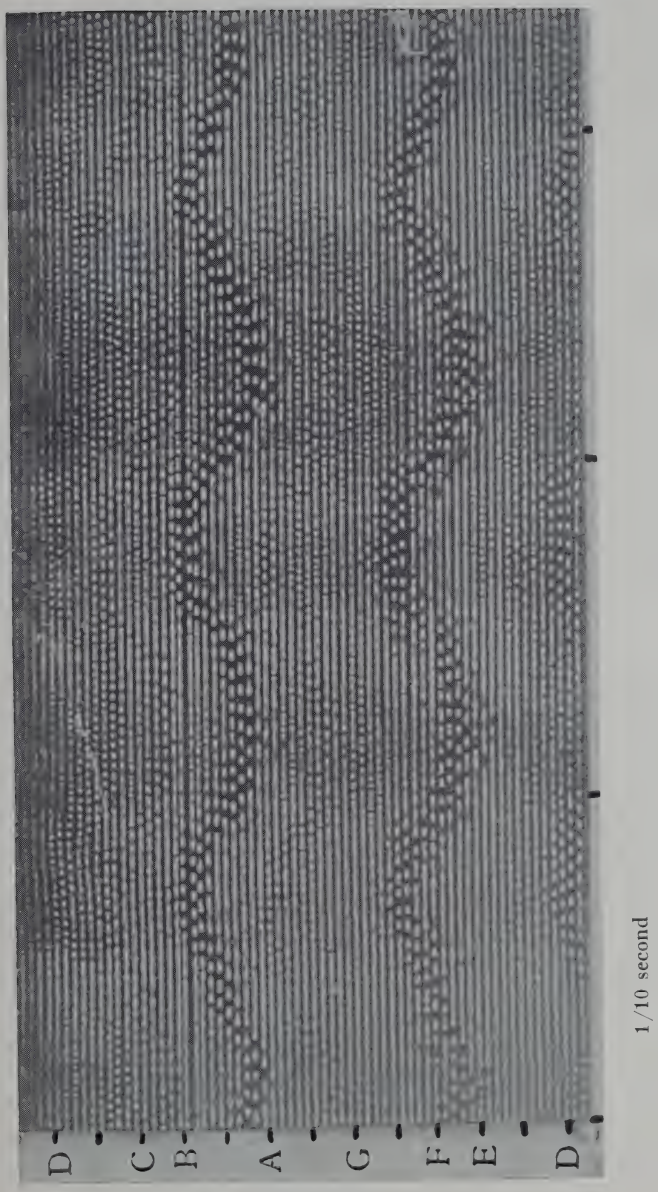


FIGURE 2

STROBOPHOTOGRAPHIC RECORD TAKEN FROM A PHONOGRAPH RECORD OF TOTI DAL MONTE'S VOICE

The variation on the scale showing a pattern of about eight times per second is not a trill, but a vibrato. The time marks at the bottom are one-tenth of a second.

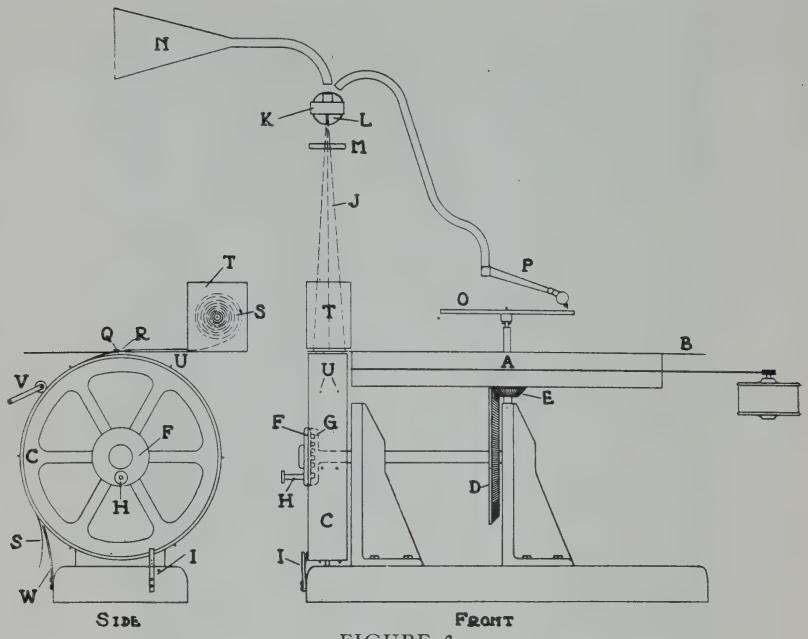


FIGURE 3

SCHEMATIC DIAGRAM OF THE PRESENT STROBOPHOTOGRAPH

photography. A slit *R* in the guide *Q* has an area which permits the vibrating beam *J* to meet one and only one hole of each of the 65 rows for each flash. The beam *J*, when not vibrating, is concentrated at the center of the slit *R*.

The photographic paper *S* is drawn from the magazine *R*, being threaded between the disc *B* and the lower platform of the guide *Q*, and through a second slit in the same platform to the drum *C*. Here the paper is gripped by sharp pins *U*, when the paper passes between the notched guide *V* and the drum *C*. A prong *W* removes the photographic paper *S*, so that any lengths may be taken without winding around the drum *C*.

For radio photography, the pin of a cone loud speaker is attached to a thread of a phonelescope, omitting the diaphragm. An electric filter is in the circuit so as to cut out all frequencies above any one of four levels, viz., 200, 400, 600, and 800 dv. This produces a less complex flashing of the light beam across the face of the slit *R*.

FORMULAE FOR USE WITH THE PRESENT STROBOPHOTOGRAPH

1. *Direct or radio photography with stationary film* in which $n$ is the frequency of the flashing light, $r$ the number of r.p.s. of the disc *B*, and $h$ the number of equidistant holes in the row photographed as dots.

$$n = r/h$$

2. *Phonograph record photography with stationary film* when the recording rate of the phonograph record is assumed to be 78 r.p.m.

$$.769\ n = h$$

3. *Direct or radio photography with moving film* in which $s$ is the speed of the photographic material and $d$ is the separation distance of the holes in the row photographed.

$$n = hr - s/d$$

4. *Phonograph record photography with moving film* in which $f$ is the distance traveled by the film in one revolution of the disc $B$.

$$.769\ n = h - f/d$$

Empirical checks of these formulae now at hand will be presented in the near future in a publication on the vibrato by the writer.

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## A PROJECTION CALIPER FOR MEASURING AT A DISTANCE

A large number of interpupillary gauges have been developed to meet the requirements in connection with fitting spectacle frames and centering lenses in front of the eyes.[1] These devices vary all the way from a simple scale in the form of a paper or celluloid ruler that the operator rests on the bridge of the nose and attempts to hold parallel and horizontal with the patient's eyes to the elaborate double-tubed gauge of the Hertel type, an improved form of which is extensively advertised in the United States at present.[2] In practically all of these devices it is necessary for the operator to look past the scale or gauge points to the patient's eyes and thus to line up two pairs of points as best he can. To avoid parallax and the errors that are likely to come from observing with one eye or with one eye at a time, the better instruments provide a separate observing tube for each eye, but still the scale is not actually in contact with the patient's eyes.

The occasion of this note is to draw attention to a wholly new type of interpupillary gauge which was patented by Mathewson and Knudsen in 1920.[3] The arrangement of this rather clever device will be clear from Figure 1. It is simply two parallel projection systems, one of which can be moved with reference to the other. They arranged the apparatus in a small thin box with a battery in the compartment, $P$, below and a mirror on the outer surface of the side, $M$. One lamp, $A$, is mounted in a fixed position 6 inches behind a cylindrical lens, $A'$. A second lamp, $B$, is mounted on one end of an arm, $C$, which at the other end carries a second cylindrical lens, $B'$. The axes of the two cylindrical lenses are placed at right angles to a line extending between them. Thus when held at a suitable distance from a screen the lighted lamps are projected as two bright, sharp, parallel lines. If the level, $L$, is pushed or pulled, these lines will be brought nearer together or farther apart. The two rods, $E$ and $F$, constitute a frame on which the system, $C$, may be moved parallel to the fixed system.

Used as an interpupillary gauge the light lines are projected directly on the patient's eyes and are moved so as to bisect the two pupils. A scale, $S$, is fixed at the edge of the window of the box so that the distance between the two optical systems can be read off directly in millimeters.

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[1]The American Encyclopedia of Opthalmology, 1916, IX, p. 6541 ff.

[2]Bausch & Lomb, Interpupillary Gauge, Bausch & Lomb Optical Publication, Rochester, New York.

[3]United States Patent Office Specification 1,327,163, dated January 6, 1920.

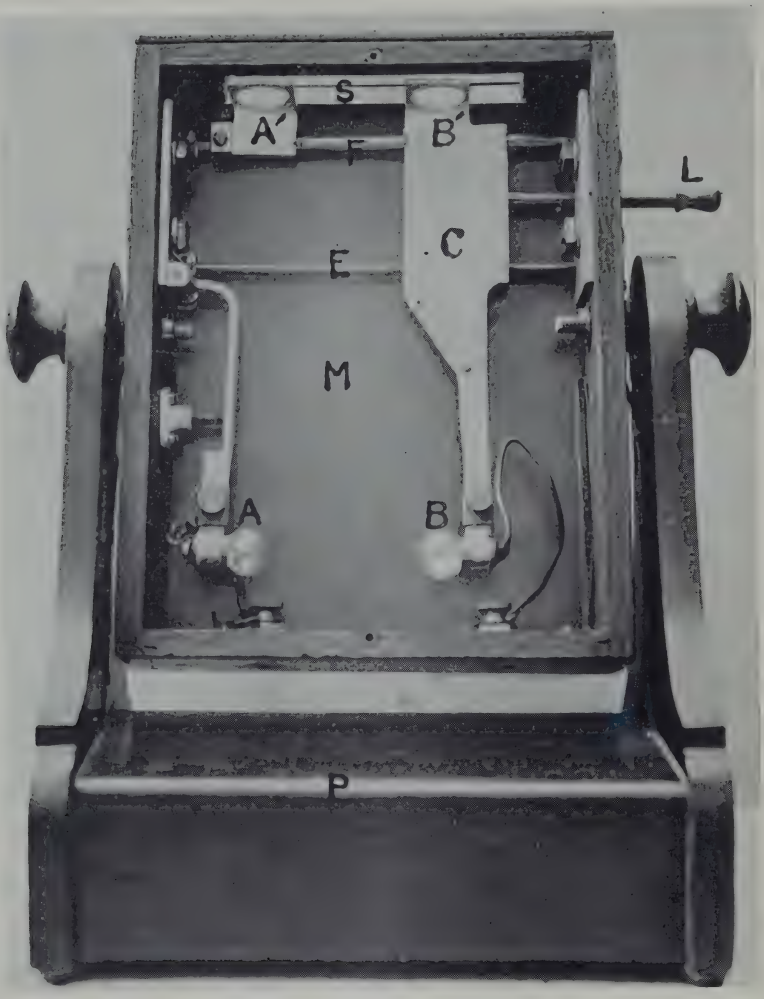


FIGURE 1
MATHEWSON PROJECTION CALIPER FOR MEASURING INTERPUPILLARY DISTANCE

Obviously this specific instrument[4] is designed just for measuring interpupillary distances. But the eyes are not the only things that cannot be conveniently or comfortably touched for purposes of measurement, and the principle of measurement by projection is a real contribution to scientific

[4]Those interested in the illustrated form should communicate with Mr. H. B. Mathewson, Palo Alto, California.

technique. With this or a similar projection caliper it is easily possible to measure with considerable accuracy objects or relations between objects without very close approach to them. An apparatus based on this principle would be very useful in the museum, zoölogical garden, and in making laboratory measurements on animals and on certain spatial features connected with their behavior. Apparently this type of device would be improved by having a partition between the two optical systems so that light from lamp, *A*, would be wholly cut off from lens, *B'*. The small projectors now available for showing single frames of standard motion picture films lend themselves fairly well to combination for such projection measurement purposes. These measurements can be made in ordinary light but of course not in bright sunshine.

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## VISUAL ILLUSIONS OF MOTION DURING TRAIN TRAVEL

The approximately stationary observer rather infrequently notices illusions of motion. The whirling windmill or lawn sprinkler seen nearly edgewise and at a distance may give the impression of turning first one way and then the other. A lace curtain swaying in front of a window screen causes the appearance of dark bands that move rapidly to and fro. Flickering light as from a fireplace or from flash signs produces apparent movement in some of the really stationary objects which are illuminated chiefly by this light. A large waterfall, a moving field of clouds, or a moving belt seen near-by induces the impression of movement in the adjoining visual areas. These represent the chief classes of illusion of motion which are to be observed by the bystander, and they do not constitute a very large proportion of his visual experiences.

However, when he is being rapidly carried by train, automobile, or airplane, practically the entire visual field is in movement. When one is sitting on the observation platform while the train goes ahead at a rapid rate, the visual field has "waterfall effect" writ large all over it. The railroad ties, crushed rock, and other features of the roadbed rush up and away, disappearing in the center of the visual field. We cross a bridge. For the instant this bridge occupies the major portion of the lower part of the field of view. In 30 seconds it has moved up to the middle of the field, that is about to the border between land and sky, and has diminished to less than 1/1000 of its former visual angle size. This rapid movement upward and away of the lower part of the field induces a rather well-marked appearance of movement up and towards me in the clouds and the upper portion of the visual field. The clouds seem to be following rapidly, as if a storm is coming up and is gaining on the train. One cannot be certain about wind direction and cloud movement during rapid travel.

If one faces in the direction of travel and looks ahead, as is characteristic when riding in an automobile, the clouds appear to recede and to be drifting over the horizon, distant mountains seem to sink down, and the whole landscape ahead tends to appear somewhat flattened. During automobile riding our bodies are jostled more and fixation is not so steady as during train travel, hence the effect when approaching the field of view is not so familiar as the experiences from the observation platform.

When one is viewing from the side of the coach, the induced motion is chiefly lateral. The upper part of the visual field, or in general one may

say that part beyond some fairly distant fixated object, seems moving in the direction of travel. If one sits at the rear end of the coach and looks fixedly at the front end, giving attention the while to the right and left peripheral fields which are moving backward, the front end of the coach seems to be retreating. The coach appears to grow longer in the direction of travel. On the other hand, if one faces backwards and watches the rear of the coach, it seems to be coming closer. This apparent crowding in and shortening of the coach may be a factor which causes discomfort to some when they ride backwards.

The so-called "waterfall illusion" thus plays a leading rôle in our visual experience of motion during rapid transit. The visual field here as at other times operates as a compensating system of experiences, one experience interpreting the other. For brightness, color, and motion its responses in different parts are relative rather than independent.

If we count the impression of the *divided field of opposed motion* just discussed as the first (1) and most important illusion of motion during train travel, there are at least three others that can be easily observed: (2) panorama experience, (3) shrinking and sinking, and (4) twisting or revolving.

Many an aviator has used the words, "I saw the ground rushing up to meet me," while for some it came too far or too fast so that they never communicated the experience. When the aviator starts taxying along the field, he at first has the distinct impression of himself moving past the objects but at higher speed and especially after attaining some altitude he has just as clear an impression of having the landscape move past him. These two impressions may be designated as the *pedestrian or slow vehicle experience* as against the *panorama experience*. When the train moves slowly, not much faster than one could run, he has the first experience; when it moves so fast that he cannot easily see objects near at hand with distinctness, he has the second or panorama impression. The pedestrian experience probably has a large kinaesthetic factor for if, during rapid travel, one rapidly moves head and eyes so as to fixate some object, the panorama impression is often temporarily interrupted.

The visual illusion of objects sinking into the ground or shrinking within themselves is very prominent from the rear observation platform as one watches the rapidly receding view. For example, linemen working on the telegraph poles seem to be sliding down the poles, or one has the alternate impression that the poles are sinking into their holes or telescoping on themselves. The impression lasts as long as the rate of change amounts to a lateral movement speed that is above the threshold for the direct perception of movement. When the objects are distant 200 yards or more, their rate of shrinking in size or visual angle is not fast enough to produce the direct appearance of shrinking or of sinking down. Why they should sink or go down rather than rise doubtless rests on the more natural interpretation for disappearance and is related to the fact that the upper part of the visual field seems to be coming forward and down. An appearance of swelling or of rising out of the ground is got from a viewpoint facing the direction of travel especially if one is actually at the very front like a motorman on an electric train.

A very striking illusion of the twisting of objects, particularly poles, fence-posts, and isolated houses near the tracks is seen especially well from the side of the coach. If one faces the window and turns his eyes far to one side so as to pick up the object (near the track) when it comes in view, and then follows it with a pursuit movement, he gets this revolving illusion most distinctly. On the general principle that an object fixated tends to be

seen as still or relatively still the interpretation here of the changing aspect that the object presents is easily and naturally inferred as movement in the object itself. Only a revolving movement would present three sides of a house successively to view. The landscape appears as a panorama (it is doing the moving), and under these circumstances if an observer "fixes" one point it has to revolve.

Some other features of the visual experiences associated with the rapid movement of the observer have been discussed long ago by Helmholtz[1] and by Wood,[2] and Gertz[3] has used train travel very interestingly as an opportunity to study the pursuit movements of the eyes. The illusions of motion discussed in this note are not mere curiosities to be toyed with and dismissed at will. True enough they are clearest when we look for them and watch them, but like most illusions they normally constitute a very living part of the visual experience of the traveler and they play their tricks on him whenever he makes judgments on the basis of visual cues.

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## FIGURE FOR THE "WINDMILL ILLUSION"

The psychologist is not infrequently asked about the motion of the lawn sprinklers that have revolving arms. Some people suspect themselves of having a subtle visual defect that produces the appearance of reversed motion, while others inform you confidently that there are two kinds of sprinklers manufactured—those that turn always in one direction and those that periodically reverse or "unwind." Sellers of windmills are of course familiar with this oddity of perceived motion. Not infrequently it is a topic for extended discussion with some customer who maintains that there is a defect in the equipment that has been installed for him. Windmills and electric fans and in fact any revolving wheel with prominent spokes and an insignificant rim may serve as the stimulus for this illusion of motion.

Figure 1 has been found a suitable diagram for illustrating this reversible perspective effect. If in the vertical ellipse the wheel appears to be facing the reader's left, then *B* is in the foreground and is represented as moving downward. If *B* appears in the background and the wheel is facing the reader's right, then the foreground *A* is moving upward. In the horizontal ellipse, if one sees the top arrow as foreground, then this is moving from left to right and he seems to see the figure from below. If the bottom arrow on this figure is viewed as foreground, then the front of the figure is moving from right to left and one seems to be looking from above the wheel. In both instances, from the observer's point of view, the movement seems to be in the so-called clockwise direction. It is therefore fruitless to attempt to describe the illusion by moving one's hand or finger. The whole trick is when front and back interchange.

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[1]Helmholtz, H. Treatise on physiological optics: III. The perceptions of vision. Ed. by J. P. C. Southall. Ithaca, New York: Optical Society of America, 1925. See p. 248.

[2]Wood, C. A. Wayside optics studies in a railway train. *Pop. Scient. Mo.*, 1892, March.

[3]Gertz, H. Ueber die gleitende (langsame), Augenbewezung. *Zsch. f. Sinnesphysiol.*, 1914, 49, 29–58.

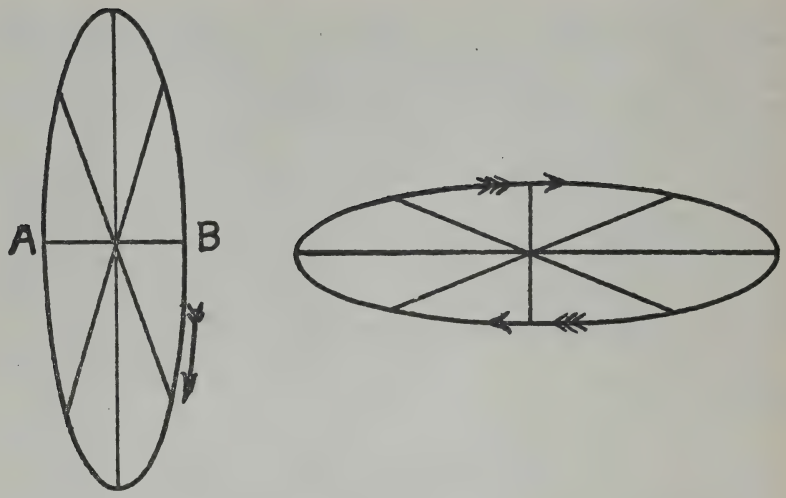


FIGURE 1

DIAGRAM FOR ILLUSTRATING REVERSIBLE PERSPECTIVE IN THE "WINDMILL ILLUSION" OF MOTION

Groups of such figures made up somewhat as the "block design" are non-effective. One can get the impression of the wheels all facing one way or the other or of their being mixed in this regard, but the impression of their revolving seems to fade out, and I think most often they tend to be interpreted simply as ellipse diagrams.

Lawn sprinklers which have an extra pair of short arms set at an angle of 45° from the vertical shaft sometimes give the impression not of revolving in one direction or the other but of unfolding or swaying back and forth in one plane. They appear vertical when they are actually one behind the other and from this position they spread out sidewise. The longer and practically horizontal arms do not give this effect as well, and I have never observed it in an electric fan or windmill. It is an illusion that applies when there are just two arms in the revolving stimulus.

Probably the lawn sprinkler is more effective as a stimulus for illusion of motion than is either the windmill or the electric fan. This is due to three factors: (1) the lawn sprinkler as a wheel is equally disposed around its support and axis of revolution; (2) it is more fragmentary in having fewer spokes and no rim whatever; (3) as it is placed in the horizontal, the eyes of the observer, also being horizontal, are at a disadvantage for perceiving depth and more easily confuse foreground and background.

This illusion of motion has been used as a test of drug effects.[1] It is treated briefly by Wallin[2] in his monograph on reversible perspective, where other references may be found. The figure which I have given is, so far

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[1]McDougall & Smith. Med. Research Council, Special Report Series, No. 56. London, 1920. A shadow windmill was employed.

[2]Wallin, J. E. W. Optical illusions of reversible perspective. Princeton, New Jersey, 1905.

as I know, original but surely has occurred to others as a simple way of describing the effect. As a diagram it resembles the Sinstenden reversing arrow figure. The ellipses which make up Figure 1 can both be seen in perspective at once in any combination but it is difficult to make both wheels appear to revolve at the same time.

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## A ONE-HANDED MOTOR TASK

For purposes of instruction and demonstration in elementary psychology it is useful to have available some simple motor tasks that are essentially novel in character, make slight demands as to equipment, and can be performed without too much noise and confusion. Stunts that meet these requirements are not particularly easy to find. The one here described has proved serviceable in small sectional groups.

FIGURE 1

ONLY THIS FORM OF LINKING OF THE TWO PAPER CLIPS IS COUNTED SUCCESS IN THIS MOTOR TASK WHICH IS PERFORMED WITH ONE HAND

The task is to take two ordinary paper clips,[1] each 1 1/4 inches long, place them on the palm of one hand and holding them both in that hand to link them together without any help whatsoever from the other hand or from any other object. The subject is to act as if he has only one hand. If he drops a clip he must pick it up with his performing hand and go ahead. Only the linkage illustrated in Figure 1 is to be scored as success. There are several ways, some much more roundabout than others, of pressing and working the clips together. At the first attempt considerable time is likely to be spent in trying various schemes. The best method seems to be that of bringing the two wide ends together and of pressing one inside the other just a little way and then of working it up and around the outside open end of wire.

Some will find it a nearly impossible task and will keenly realize how dependent they are on the joint performance of both hands. Average serious performers will require from start to finish 1 to 2 minutes at first. After a few trials it can be done by the most skilful in 10 seconds or perhaps even a shorter time. The learning curve will of course be irregular because the task is short and the chance factors are prominent in this case. The average individual will find his "left-hand performance" about twice as difficult, stating the matter in time score. As usual it will be necessary to see

[1]The paper clip used is sold under the trade name of Royal Gem No. 1.

that the clips used have not been accidentally or for the sake of "preparedness" bent from their regular form. The diffuse motor activity which the task calls forth is a useful exhibit.

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## ON VIEWING THE SPECTRUM THROUGH A PRISM

The spectrum is ordinarily exhibited by passing a beam of light through an equilateral prism and causing the dispersed rays to fall on a screen beyond. For this purpose direct sunlight or some fairly concentrated source is required. The natural response of a child is to look into or through the prism itself, and this common childhood experience is well worth repeating for adult psychological purposes.

There is no more simple way of securing a vivid experience of saturated color than to rest a long prism flatly against the bridge of the nose as if with base up and to look down and through it at a heterogeneous field of objects illuminated by direct sunlight. The field of view will of course be distorted; it is difficult even to read large print through an equilateral prism. The field will appear rounded, high in the center, and sloping off sharply at the right and left. But the most challenging thing is the gorgeous color display.

The visual experience which comes from looking at a landscape in this manner is a worthy object of study. I am not acquainted with any other equally simple perception experiment which yields richer results.

1. The landscape appears brighter than when seen without the prism in the ordinary mixed light. If it is cloudy or say after sunset, the view through the prism gives the impression of full sunlight; there is the effect of stronger highlights and deeper shadows. This seems chiefly due to the rather large amount of yellow that is visible at the bases of all isolated objects, while blue tops all objects on the horizon. It is due in part to the adaptation factor and in part also to general interpretation from the experience that bright colors appear as a rule on bright days.

2. Only the edges of things seem to give off colors. The white flagpole appears evenly white throughout its length but its base ends on a cushion of red and yellow while its tips look like a bright blue flame. The rough tree-trunk ends at the ground like the flagpole but has a mottled spectrum appearance up to the branches and these that are fairly horizontal have very colorful edges. A fly on the window appears blue and seems to have a violet shadow with a second shadow lower down of yellow. Unconsciously we have a great deal of color-mixing in the field before us. For example, each point on the white body of the flagpole gives a full spectrum with violet at the bottom and red at the top. There are very many such points arranged one above the other, and the small spectra cast on the retina thus overlap in such a manner as to cancel out completely all the chroma. When the series of points comes to an end at the top or bottom borders of the object, there occur on the retina a series of similar partial spectra ranged side by side, which are not cancelled by other spectra from the same reflection source of light. This provides the basis for prominent color bands which appear in the ordinary landscape and is the reason why homogeneous surfaces appear unchanged through the prism.

3. The spectra which are seen are partial and, when these parts are combined, the colors are usually arranged out of order with yellow at the

bottom and blue at the top. (A) Solid objects on a lighter field, e.g., a wide board placed across the window shows at the bottom next the object vivid sunset red, and below this red a narrow band of orange, and then a wide band of yellow, below the yellow, blue or gray sky. At the top of the board there is first a faint but fairly broad band of dark violet, and above this a graded series of saturated blues ranging from dark to light and then sky. A fairly definite line seems to separate the spectral blues from the sky-blue. No green appears either below or above. From below, the overlapping or color cancellation takes violet, blue, and green, that is, up to the yellow, while from above it takes red, orange, yellow, and green, that is, down to the blue. If the board or dark band is narrow, say about 2 inches wide, and is seen from a distance of 8 feet, then one gets a pseudo-spectrum with blue at the top and yellow at the bottom and faint dark violet to purple in the middle, but there is no green. Two such dark bands placed across the window, with an open space about 2 inches wide betwen them, give of course a perfect spectrum with green in its proper place and with red at top and violet at bottom. What we have here is a horizontal line source. The effective overlap of the spectra is half the complete spread of each spectrum, and as green comes in the middle it gets caught from both sides. (B) A light object on a dark field, e.g., a white sheet of paper on a blackboard, gives yellow, orange, and red consecutively above with blue and violet (deep violet because of the overlapping with the blackboard) below the sheet. Green does not distinctly appear until the sheet is made quite narrow. White fleecy clouds through the prism have red tops while the trees on the horizon are a vivid blue. A lattice with bands of different width and spacing across the window and a vertical series of horizontal chalk marks on a blackboard give striking color-mixing results.

4. Viewing the landscape through a prism submerges details and accentuates masses of light and shade. It is a way of studying the composition of any view or picture. Rotating the prism about its axis expands or compresses the field. Viewing with the prism held with its axis vertical gives a varigated appearance to the horizon and produces pairs of color bands for all normally vertical objects such as tree-trunks, fence-posts, etc.

In an early publication Wundt[1] gives a figure showing the eye looking through the prism but his discussion concerns only the usual single spectrum from a point source. He does not describe this as a method of demonstrating spectral colors at any time without direct sunlight or artifical source, or of seeing many partial spectra and the pseudo-spectrum, or of color-mixing by superposition,[2] or of submerging the details of the landscape.

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## A METHOD FOR OBSERVING THE POSITION AND DIRECTION OF MOVEMENT OF THE EYE

To determine the direction of movement and the fixation position of the eyes is a matter of great difficulty without the use of elaborate photographic

[1]Wundt, W. Lehrbuch der Physiologie des Menschen. Stuttgart, 1878. See Fig. 129, p. 648.

[2]Helmholtz, H. Treatise on physiological optics: II. The sensations of vision. Ed. by J. P. C. Southall. Ithaca, New York: Optical Society of America, 1924. See p. 161.

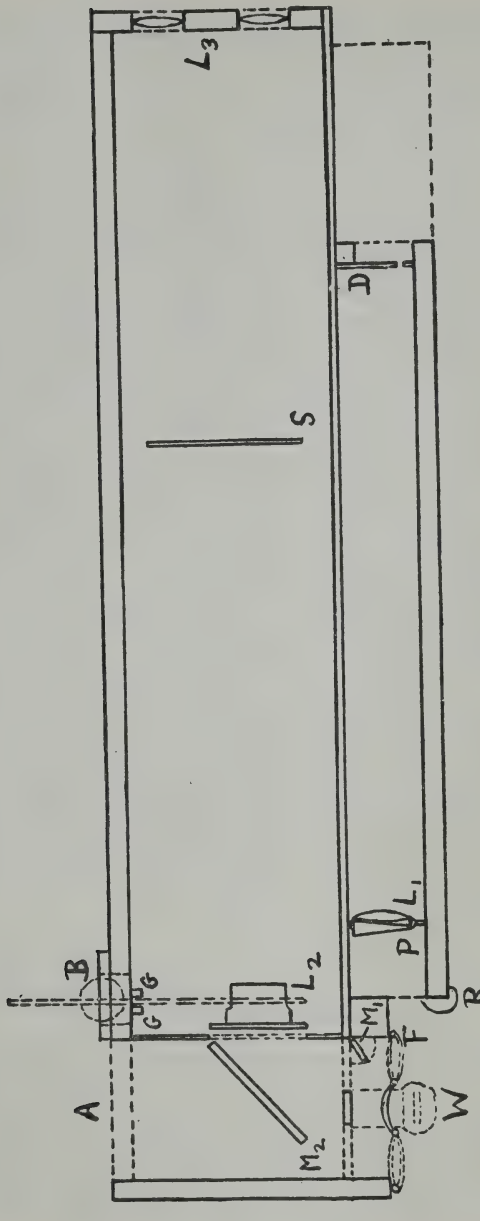


FIGURE 1

Apparatus for Observing Position and Movements of the Eye, Shown in Horizontal Section Through its Center

apparatus, such as that employed by Dodge, Judd, C. T. Gray, etc. But by the following method the movements and fixation positions in reading or in perceiving a series of dots, letters, etc., may be observed directly upon a scale.

The apparatus is simple and easily constructed. It is contained in a wooden box about 30 inches long and 5 inches high, as shown in horizontal section through its center, in Figure 1. Light passes through the small square aperture in the diaphragm *D*, and is focussed and refracted by the lens $L_1$ and the prism *P* to fall on a small vertical mirror as $M_1$. From here it is reflected to form an image on the surface of the cornea of the subject, seated with his eyes and nose in the spectacle frame *F*. The "bright spot" thus obtained is reflected from the half-silvered mirror $M_2$, and focussed by the lens $L_2$ to form a magnified real image on the graduated glass screen *S*. The movements of this image are observed, still further magnified, through the lenses at $L_3$.

In order to obtain fairly even illumination of the "bright spot," light from the sky is reflected from the mirrors $M_3$ and $M_4$, as shown in Figure 2, and made to pass horizontally through the aperture in D. Direct sunlight is not employed. The diaphragm *D*, the lens $L_2$ (Figure 1), and prism *P*, and the screen *S* are mounted on stout cardboard which passes through slits in the lid of the box, and can thus be suitably adjusted, both horizontally and vertically. The mirror $M_1$ swivels about a vertical axis held in position by a lump of plasticine. $L_2$, a Ross (London) 9 D camera lens, must be rapidly adjustable in any direction at right angles to that of the beam of light; in this apparatus it is secured to a light rod carried in a ball joint *B*. The rod slides between grooves *G*, *G*. The lenses at $L_3$ are convex spectacle lenses of 3.25 D; the eyes can be used alternately for observation.

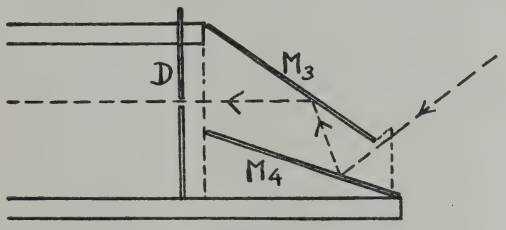


FIGURE 2

RIGHT-HAND END OF APPARATUS IN VERTICAL CROSS-SECTION, SHOWING MIRROR ARRANGEMENT FOR REFLECTING SKY LIGHT THROUGH DIAPHRAGM *D*

The head of the subject rests against the pad at *R*, and is kept in position by *W*, an adjustable mouth-piece of wax on which has been taken an impression of his teeth. Chin and forehead rests, not shown in the figure, can also be used. The subject sees clearly, through $M_2$, any reading or other material placed before the aperture *A*. The position and movements of the bright spot on his right eye can meanwhile be observed on the scale on *S*.

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## THE RELATION OF LENGTH OF MATERIAL AND NUMBER OF REPETITIONS NEEDED TO LEARN

As a result of his investigations during the years 1883-1884, Ebbinghaus[1] formulated a law of relationship between the length of material to be learned and the number of repetitions necessary to learn the material to a point of first errorless reproduction. This law has been taught for many years by various authors. Thus Pillsbury[2] says, "As the number of syllables in a series increases, the number of repetitions required for learning increases much more rapidly than in proportion to the increase in the number syllables." In a word then, according to Ebbinghaus, there is a disproportionately larger increase in repetitions needed as syllables of a series are increased.

Meumann,[3] on the other hand, formulates a law which is just the reverse of the Ebbinghaus statement. Thus Meumann says, ". . . the learning of large masses of material demands relatively fewer repetitions than the learning of smaller masses of material." In other words, Meumann states that, as the syllables in a series are increased, there must be a disproportionately small increase in the number of added repetitions.

The writer wishes to suggest that neither of the above statements is true, although each holds within certain limits.

The true statement, we believe, is that the Ebbinghaus law holds up to a certain point in the increase of learning material while the Meumann law holds for learning material beyond this point. Let us look at the data presented by these investigators—data upon which they base their respective laws. Tabulation 1 presents the data. It is very unfortunate that there are such gaps between different learning series considered in each case and it is equally unfortunate that the series were not extended beyond thirty-six in number. In all cases S stands for syllables and R represents repetitions.

TABULATION 1

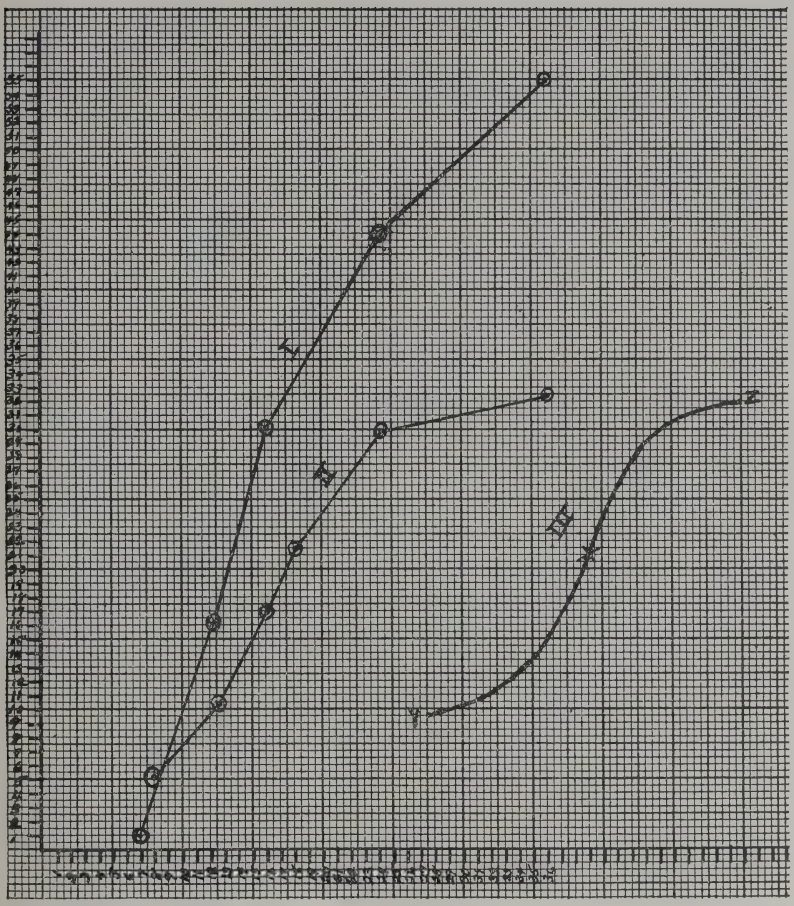
| Ebbinghaus Data | | Meumann Data | |
|---|---|---|---|
| S | R | S | R |
| 7 | 1 | 8 | 5.2 |
| 12 | 16.6 | 12 | 10.4 |
| 16 | 30.0 | 16 | 17.0 |
| 24 | 44.0 | 18 | 21.5 |
| 36 | 55.0 | 24 | 30.0 |
| | | 36 | 32.5 |

It is obvious that we cannot say that S and R have any linear relationship between them, hence they are not proportional. In the accompanying graph Curve I represents the Ebbinghaus S—R relationship whereas Curve II gives the Meumann relationships. Curve I substantiates the Ebbinghaus law up to and including the sixteen-syllable series. The reader will note a significant inflection of the curve from that point onward.

[1]Ebbinghaus, H. Memory. (Trans. by H. A. Ruger.) New York: Teachers College, Columbia Univ., 1913.

[2]Pillsbury, W. B. Fundamentals of psychology. (Rev. ed.) New York: Macmillan, 1922. See p. 371.

[3]Meumann, E. The psychology of learning. (Trans. by J. W. Baird.) New York: Appleton, 1913. See p. 255ff.




In Curve II, the Meumann curve, the eighteenth-syllable addition appears to mark a critical point in the direction of the curve. Up to and including the eighteen-syllable series the Ebbinghaus law is certainly substantiated, but beyond this point the Meumann law would seem valid.

Curve III is a hypothetical curve which expresses the writer's notion of the actual situation. The *x* on the curve marks the point of inflection. Up to this point the curve direction has been such as to express the Ebbinghaus law, but beyond this point the Meumann law is expressed. Just where the point of inflection actually occurs is a question which careful experimental work alone can answer. The work reported by both Ebbinghaus and Meumann is much too scanty to justify any positive statement on this point. It does not take much imagination to see Curve III in Meumann's

curve (Curve II). One gains the suggestion from a study of Curve II that the point of inflection of the curve corresponds to a syllable-series of eighteen syllables.

The same facts or relations can be stated in another way. In Tabulation 2 we present the Ebbinghaus and Meumann data in terms of repetitions per syllable.

TABULATION 2

MEUMANN DATA:

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| 8 | syllables | require | 5.2 | repetitions | or | 0.65 | repetitions | per | syllable |
| 12 | " | " | 10.4 | " | " | 0.87 | " | " | " |
| 16 | " | " | 17.0 | " | " | 1.20 | " | " | " |
| 18 | " | " | 21.5 | " | " | 1.19 | " | " | " |
| 24 | " | " | 30.0 | " | " | 1.25 | " | " | " |
| 36 | " | " | 32.5 | " | " | 0.90 | " | " | " |

EBBINGHAUS DATA:

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| 7 | syllables | require | 1.0 | repetitions | or | 0.14 | repetitions | per | syllable |
| 12 | " | " | 16.6 | " | " | 1.39 | " | " | " |
| 16 | " | " | 30.0 | " | " | 1.88 | " | " | " |
| 24 | " | " | 44.0 | " | " | 1.83 | " | " | " |
| 36 | " | " | 35.0 | " | " | 1.53 | " | " | " |

Just how far the Meumann law will hold is an interesting question. It is possible that when the syllable series has become very long the curve may again take the form that it has from $y$ to $x$ in Curve III in our graph. Thus the Ebbinghaus formulation would again become valid. The writer has a suspicion that such would be the case if the learning series became sufficiently long. Again, we must wait upon further experimental study to prove or disprove this speculation.

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# BOOKS

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KELLEY, T. L. *Crossroads in the Mind of Man: A Study of Differentiable Mental Abilities.* Stanford: Stanford Univ. Press, 1928. Pp. vii+238. $4.00.

1. *Heterogeneity.* In Spearman's *The Abilities of Man, Their Nature and Measurements,* which I reviewed in *Science,* vol. 67, March 1928, pp. 244–248, there is a differentiation of the scores on various tests into a general factor or part $g$ which is taken as the general intelligence, and a factor or part specific to each test. Spearman specifies that his tests must not overlap except in respect to $g$ and in case there is other overlapping he suggests that the tests be modified so as to eliminate that overlap.

Kelley assumes a plurality of general factors or parts (it is a question whether the term factor is appropriate to parts which are added together, though it is frequently used in such cases in general parlance) so that the $Nn$ scores $x_{ik}$ on $N$ tests taken on $n$ individuals may be written $x_{ik}=c_i a_k+k_i b_k+\ldots+e_{ik}$ where $a, b, \ldots$ denote general factors $a_k, b_k, \ldots$ the values of those factors for the $n$ different individuals, $c_i, k_i, \ldots$ constants that go with the test but do not vary from individual to individual; and $e_{ik}$ with $i=1, 2, \ldots, N$ and $k=1, 2, \ldots, n$ are the $Nn$ special or specific abilities of the $n$ individuals in the $N$ tests.

It is the problem to determine the relative importance of the general factors $a, b, \ldots$ and the specific factors $e_i$, (which through the use of the coefficient of reliability may be resolved into two specific factors, one chance and the other not-chance) for any group of tests on any group of individuals. The first illustration which the author gives is for nine tests: 1° Reading-Speed, 2° Reading-Power, 3° Arithmetic-Speed, 4° Arithmetic-Power, 5° Memory-Words, 6° Memory-Numbers, 7° Memory–Meaningful-Symbols, 8° Memory–Meaningless-Symbols, 9° Space-Power, resolved into six general factors which he calls traits, namely, $\alpha$ Heterogeneity (Maturity, Race, Sex), $\beta$ Verbal, $\gamma$ Number, $\delta$ Memory, $\epsilon$ Spatial, $\zeta$ Speed, and the nine specific factors which are subdivided into chance and not-chance. We thus resolve nine tests into twenty-six mutually uncorrelated elements.

This is very much super-Spearman, but is a logically necessary extension of the same method of thought, viz., resolution into the general and the specific. Why, forsooth, should one seek to modify a test or group of tests so as to eliminate other overlapping and thus have only a single general

factor; why not admit a plurality of general factors as a tool of the psychologist's trade—particularly when analytically and conceptually, if not actually, one can as well in the first place so choose the tests as to eliminate all overlap[1] and thus the single general factor? When I say that Kelley's book is very much super-Spearman I do not mean to imply that it is based upon Spearman's book; for the dates of issue are so near together and both require so much detailed arithmetical work that Kelley's could not have been based upon Spearman's—their contacts must have been in the remoter literature.

The author makes it clear that his general factors or traits are group phenomena. He does not go on, any more than did Spearman, to assign to each individual in the group examined his appropriate rating on each general and specific factor. Indeed, as I showed in my review in *Science,* and further in a mathematical article on "Hierarchical Correlation Systems" in the *Proceedings of the National Academy of Sciences,* vol. 14, March 1928, pp. 283–291, this problem is highly indeterminate—not in the sense that the tests are unreliable in that the individuals do not do themselves perfect justice (neither more nor less), but in the sense that the conditions imposed by the theory of the general and specific factors are not sufficient to determine the individual values, even supposing the scores made on the tests to be absolutely reliable.

Now this indeterminateness has its advantages in addition to its disadvantages. For example, nobody could expect to assign to the individuals that part of their special or general abilities due to chance and that remaining part due to not-chance without a special investigation of the reliability of each individual on each test over and above the mean or average reliability of each test in the group of individuals. Yet the reliability of the individual pupil in his subject was one of the things best known to the old-fashioned teacher using the Socratic method with his class and keeping a marking-book, and maybe reliability was one of the characteristics of the individual that went heavily into the estimate of that individual by that teacher. Is reliability a general or a specific character of the individual?

For the resolution into a single general and specific factor it is necessary that all the tetrads $r_{ij}\ r_{kl}—r_{ik}\ r_{jl}$, where $i, j, k, l$ are all different, should vanish, and conversely, if they do all vanish (and certain inequalities are satisfied), the resolution is really possible, irrespective of whether or not the regressions are linear (because in the analysis we use only the intercorrelations, $r_{ij}$, and the question of linearity of regression takes us back to the scatter diagrams involving the individual values scored by the $n$ persons on the $N$ tests). If nine tests are to be resolved into six general and nine specific factors, we do not know the general conditions that must

[1]Such a resolution requires the same number of general factors as there are variables but no specific factors. If the system of variables is degenerate in that one or more of them can be expressed linearly in terms of the others, a smaller number of general factors is required, namely the number of independent variables. If one wishes to eliminate chance he may operate with the correlation coefficients corrected for reliability. Moreover, the general factors may be taken as linear combinations of the original variables. See my articles in *Science* and the *Proceedings of the National Academy of Sciences* cited in the text. Once we have arrived at a set of scores $a', b', \ldots$ with all the intercorrelations zero, if we start from them to determine the resolution, we may consider them all as generals or all as specifics or any number of them as generals and the rest as specifics.

be met by the 36 coefficients $r_{ij}$. The author does not in his book attempt to determine the conditions.

2. *Number.* Let us look at the problem. We may for simplicity of notation and without any loss of generality assume that the scoring of the test $x_i$, is so scaled that the standard deviation of the $n$ marks $x_{ik}$ ($k=1, 2, \ldots n$) is unity and that the general factors also all have unit standard deviations. Then

$$r_{ij}=\Sigma_k x_{ik}x_{jk}=c_ic_j+k_ik_j + \ldots, \text{ six terms,}$$

or in general as many terms as there are general factors. There are $N(N\text{-}1)/2$ intercorrelations $r_{ij}$. If there be $G$ general factors there are $NG$ constants $c_i$, $k_i$. . . . We should expect that these constants could be so chosen as to satisfy the conditions in the above equations when $NG$ is greater than or equal to $N(N\text{-}1)/2$ except for 1° the necessity of providing, presumably by inequalities rather than by equations, that the solution be real,[2] and 2° the necessity of avoiding counting as independent equations those that are really interdependent.

In case $N=3$ and $G=1$, $N(N\text{-}1)/2=3$ and $NG=3$. We expect that there is the possibility of resolving three variables into one general and three specific factors. Kelley proves that this is possible (p. 41), apart from the conditions of inequality. In case $N=4$ and $G=1$, $N(N\text{-}1)/2=6$ and $NG=4$. We should expect that there would be two supplementary conditions, and there are as a matter of fact two independent tetrad equations. In case $N=4$ and $G=2$, $NG=8$ and we should expect that the resolution of four variables into two general and four specific factors was always possible (always provided that the appropriate inequalities were satisfied) and was always possible in an infinity of ways, and the author proves that this is the case (pp. 52–5).

If $N=5$ and $G=2$, $N(N\text{-}1)/2=10$, $NG=10$ and we should expect that it were possible to resolve five variables into two general and five specific factors. But such is not the case; a conditional equation, the pentad equation which the author derives, (p. 58)[3] is necessary. This means that our

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[2]For example, if three tests are to be resolved into general and specific factors, we must have the product of each two of correlation coefficients less than the third. Thus in Kelley's table (p. 100) tests 4, 5, 6 give $r_{45}=.1487$, $r_{46}=.2893$, $r_{56}=.7169$ and the resolution into a single general and three specific factors is imaginary because $r_{46}r_{56}$ is greater than $r_{45}$; but 9, 5, 6 give $r_{95}=.4361$, $r_{96}=.4351$, $r_{56}=.7169$, and are resoluble. Why should there be a single factor of general significance in Space-Power, Memory-Words, Memory-Numbers, but not in Arithmetic-Power, Memory-Words, Memory-Numbers? Does the mathematical difference between a real and an imaginary solution imply a similar mental difference? Or does it rather imply the lack of adaptation of the mathematical method to the psychological problem? And is it not really true that the three tests must in both cases depend on a number of general characteristics or abilities?

[3]The author states that if the pentad equation is satisfied, the resolution is possible; but he proves only the converse. The theorem as stated is untrue. The pentad equation is identically satisfied if $r_{12}=r_{13}=r_{23}=0$; but the resolution of five variables into two general and five specific factors is in this case in general impossible. For here of the three variables $x_1=c_1a+k_1b+e_1$, $x_2=c_2a+k_2b+e_2$, $x_3=c_3a+k_3b+e_3$, at least one must have no general factor present and be entirely specific. If this one be called $x_1=e_1$ we must have

$N(N\text{-}1)/2=NG=10$ equations are not independent. In fact, they cannot be, because if the resolution were possible in one way it would be possible in an infinity of ways as the two general factors may be replaced by appropriate linear combinations of themselves, or, to put it in the simplest fashion, the total general factor, $c_1a+k_1b$ in $x_1$ may be taken as a new general factor $a'$ with $c'a'=c_1a+k_1b$ and a new general factor $b'$ with $k'b'=xa+yb$ may be determined so that the correlation between $a'$ and $b'$ is zero (or $c_1x+k_1y=0$), and then $a'$ and $b'$ may be substituted throughout for $a$ and $b$.

If $N=6$ and $G=3$, $N(N\text{-}1)/2=15$, $NG=18$ and we should expect that the resolution of six variables into three general and six specific factors were always possible, except for the difficulty of double counting which appeared in the case $N=5$, $G=2$. Now if the resolution is possible in any way with three specified general factors $a$, $b$, $c$, it remains possible when $a$, $b$, $c$ are replaced by three new general factors $a'$, $b'$, $c'$ obtained from $a$, $b$, $c$ by transformations analogous to rotation of axes in 3-dimensional geometry. There are $\infty^3$ such rotations. We must therefore subtract from $NG=18$ the figure 3, which leaves 15. If there are no further enumerational troubles, we should find that six variables may be resolved into three general and six specific factors in $\infty^3$ different ways (appropriate inequalities being satisfied).

There is perhaps no more tricky part of mathematics than that involved in counting equations and variables to determine whether or not the equations can in general be solved. Today this kind of mathematics is, among pure mathematicians, taboo except as a heuristic device. I do not wish to say that six variables may be resolved as stated above. The indications are that they may be and we should look for a proof of the proposition—but not here.[4] Let us however state the general proposition that this counting of degrees of freedom would indicate. We have $N(N\text{-}1)/2$ equations for $r_{ij}$ in the $NG$ constants $c_i$, $k_j$. . . . If the resolution is possible in one way the $G$ general factors may be modified by an orthogonal transformation among themselves which has $G(G\text{-}1)/2$ degrees of freedom. (It is assumed that $G$ is not greater than $N$.) Hence if we get one solution we get $G(G\text{-}1)/2$ and we must compute

$$N(N\text{-}1)/2-NG+G(G\text{-}1)/2=T$$

If $T$ is greater than zero there should be no solution except with $T$ new additional equations of condition; if $T$ equals zero there should be a "unique"[5] solution except for the group of orthogonal transformations among

---

$r_{14}=0$ and $r_{15}=0$, and in general they will not vanish. It is possible that appropriate inequalities not stated by the author would take care of such a case.

[4]Even if this count of constants is not right some count similar to it would be correct and with apologies we may proceed as though our count were right to illustrate the principles involved.

[5]We use quotes on unique because it is only in a very special sense that uniqueness can exist. Unless the inequalities are satisfied there is no real solution. If they are satisfied there may be several distinct solutions so far as the count of constants can indicate—the equations being non-linear. We explain uniqueness a bit more later on. In respect to the use of reliability coefficients, I believe they should count rather as inequalities than as equalities though Kelley implies, perhaps unintentionally, that they count as equations (bottom p. 105); they would become equations if we assumed that there

the general factors; if $T$ is negative, there should be an infinity of solutions, with $T$ degrees of freedom, over and above the freedom introduced by the permissible transformations among the $G$ general factors.

Is the theorem true (always provided that the necessary inequalities among the coefficients $r_{ij}$ are satisfied so that the solution is real and not imaginary)? I do not know. But it seems to be true for those special cases treated by Kelley. Let us try it on his first example where $N=9$ and $G=6$. We have

$$T=36-54+15=-3$$

Here $T$ is negative and it would seem as though nine variables could be resolved into six "traits" and nine specific factors in more than the number of ways available through orthogonal combinations of one set of general factors into another set. Let us try it on his example of thirteen tests and seven general factors (p. 122).

$$T=13\cdot12/2-13\cdot7+7\cdot6/2=78-91+21$$

It appears that $T$ is positive but that $N(N\text{-}1)/2-NG$ is negative. That is to say, if we note that there are 78 coefficients $r_{ij}$ giving 78 equations involving the 91 unknowns $c_i$, $k_i$, . . .we should infer that there are so many more unknowns than equations that a solution is possible; but if we reflect that for each solution obtained we should have a group of $\infty^{21}$ generated by orthogonal transformation of seven general factors among themselves, we may well doubt whether there are any solutions. Yet Kelley gets a solution and I understand that he thinks it is unique. What does this mean? First, our theorem may be wrong, as is not unlikely. Second, there may have been additional relations among the 78 correlation coefficients which would have increased the number of equations without our knowing it (tetrad equations or pentad equations or the like), and it is pretty certain that there are such equations which are satisfied within their probable error. Third, he may merely have obtained a solution within a limited error which is large enough to permit a non-solution to appear as a solution—which is much the same as the second way out. To illustrate: The three equations

$$15x+23y=38, \qquad 40x-7y=23, \qquad 20x+8y=29$$

in the two variables $xy$ have no solution. The first two give $x=1$, $y=1$; the first and last give $x=363/340=1.07$, $y=.95$; the last two give $x=467/460=1.02$, $y=23/25=.92$, to two decimal places. The mean value of $x$ is 1.03 and of $y$ is .97, and if the error of determination of $x$ and of $y$ is about .04 we could well enough say that the equations had a solution. Or fourth, he may have imposed by his method of obtaining the solution some additional restrictions of some sort which make the solution possible and unique.

Consider the case of nine tests with $N(N\text{-}1)/2=36$. For the resolution into one general factor and nine specific factors (Spearman's case) we need $36-9=27=9\cdot6/2$ additional equations and there are just this number of independent tetrads. For a resolution into two general and nine specific factors we should need $36-9\cdot2=18$ equations of condition, except for the fact

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were no other specific factors than chance. In that case the count of constants and equations would show that for $N$ linearly independent tests (independence being judged on the basis of the correlation coefficients corrected for attenuation) there would in general be needed $G=N$ general factors and none specific, and such a resolution can always be made if the corrected coefficients form a consistent or possible set.

that each resolution implies a group of $\infty^1$ resolutions so that we might need 19 additional equations of condition. As a matter of fact every 5 variables being resolved into two general and five specific factors, and there being $\frac{9\cdot8\cdot7\cdot6\cdot5}{1\cdot2\cdot3\cdot4\cdot5}=126$ such sets of five that may be selected from nine, there are in fact 126 pentad equations that are satisfied. How many of these are independent we do not know, nor do we know that there may not be other conditions to be satisfied. The problem is a mathematical problem which must be solved before we know just where we are at in respect to the resolution.

Let us go on to $N=9$ and $G=3$. Here we have 36 equations in 27 unknowns. Moreover for each set of possible unknowns there are $\infty^8$. In other words we can assume if we wish that $x_1$ contains only one general factor, $x_2$ only two, and $x_3$ to $x_9$ the three factors. We should then need to get a solution involving only 24 unknowns and we should need 12 additional equations of condition. For $G=4$ we have 36 equations and 36 unknowns, but 6 of these seem to be supernumerary, leaving 6 independent equations to be added. For $G=5$, there are 45 unknowns with 10 supernumerary and we still need one equation of condition. For $G=6$, we seem to have, as above stated, more unknowns than we need even after allowing for supernumerary ones.

3. *Memory.* So much by way of illustration of the method of counting equations and unknowns. Much tightening up of the reasoning needs to be done before any substantial confidence can be placed in the method. But, still assuming the count to be sound we find that $T=0$ when

$$G=\tfrac{1}{2}[2N+1-\sqrt{8N+1}]$$

and in this case there would be a solution with no degrees of freedom other than those implied in the orthogonal transformation. If $N=1$ we have $G=0$, but this case is trivial. We can, if we please, think of one variable as resolved into as many traits as we wish, but the problem is grossly indeterminate as it must be in any case in which the number of unknown traits exceeds the number of variables. If $N=2$, $G=.4+$ and we can make the resolution with one general factor. This is Kelley's proposition of p. 38.

The resolution, however, should not be unique. In fact suppose

$$x_1=c_1a+e_1, \qquad x_2=c_2a+e_2, \qquad r_{12}=c_1c_2$$

Now if we take any value whatsoever for $c_1$ and write $c_2=r_{12}/c_1$ we have

$$x_1=c_1a+e_1, \qquad x_2=r_{12}a/c_1+e_2$$

The amount of "trait $a$" in $x_1$ is $c_1$ and in $x_2$ is $r_{12}\ a/c_1$ with $c_1$ arbitrary. This is what is meant by saying that the solution is not unique. Of course as applied to the $n$ individuals it is highly indeterminate even when $c_1$ is given any specified value. If $N=3$, $G=1$ and the solution is unique. There is perhaps no need to dwell so much on the illustration, particularly as the count is not sure. But if we examine the case of $N=3$ in comparison with that of $N=2$ we do see what is meant by a solution that is unique or not unique with respect to the amounts of the general factor that enter into the variables.

And as psychologists are ordinarily not mathematicians they seem to have a great difficulty in seizing just what Spearman and Kelley mean. There is also the possibility that these authors themselves are still in the progressive groping stage of their investigations, as is often the case with persons breaking new scientific ground, and may find my tedious and iterated illustration not too elementary for themselves as I feel confident it is not

for many of their readers. I will therefore go on to the case $N=4$. Here $G=1.7$ which may be interpreted as meaning that for $G=1$ additional conditions are necessary to make a solution possible (Spearman's tetrads must vanish), but for $G=2$ the solution is possible though not unique.

Kelley treats this case, but let us treat it again.[6]

$$x_1=c_1a+e_1 \qquad x_3=c_3a+k_3b+e_3$$
$$x_2=c_2a+k_2b+e_2 \qquad x_4=c_4a+k_4b+e_4$$

We have started with $k_1=0$ as is possible without essential limitation of the generality. As always, I assume all standard deviations of $x_1$ and $a$, $b$ to be unity.

$$r_{12}=c_1c_2 \qquad r_{23}=c_2c_3+k_2k_3$$
$$r_{13}=c_1c_3 \qquad r_{24}=c_2c_4+k_2k_4$$
$$r_{14}=c_1c_4 \qquad r_{34}=c_3c_4+k_3k_4$$

There are seven unknowns and six equations. We may take one of the unknows, viz., $c_1$ as arbitrary. Then

$$c_2=r_{12}/c_1, \qquad c_3=r_{13}/c_1, \qquad c_4=r_{14}/c_1$$

These may be substituted in the last three equations to get

$$k_2k_3=r_{23}-\frac{r_{12}r_{13}}{c_1^2}, \quad k_2k_4=r_{24}-\frac{r_{12}r_{14}}{c_1^2}$$

$$k_3k_4=r_{34}-\frac{r_{13}r_{14}}{c_1^2}, \quad \frac{k_3}{k_4}=\frac{r_{23}-r_{12}r_{13}/c_1^2}{r_{24}-r_{12}r_{14}/c_1^2}$$

The fourth equation is had by dividing the first two. The third and fourth equations are one quadratic and one linear. The solution is plain algebra, but $c_1$ remains arbitrary except as limited by inequalities that shall make the solution real. Next there is the additional arbitrary element which results from taking $a'$, $b'$ as $a\cos q+b\sin q$, $-a\sin q+b\cos q$ with $q$ any angle. And finally as for the individuals $a_k$, $b_k$, *and the* $e_{ik}$ are all indeterminate even for any specified choice of $c_1$ and of $q$.

Now for $N=9$ we have $G=5.2$, meaning that 5 general factors will not do and that 6 will more than do. We found above that $T=-3$ when $N=G$ and $G=6$. We therefore suspected that there should be three constants arbitrary in the same sense as $c_1$ in the immediately preceding illustration in addition to the 15 constants involved in the orthogonal transformation on the 6 general factors. And hence again we raise the question as to how the author is entitled to his belief (p. 84) that he has a unique solution.[7] He has a table of the constants $c_i$, $k_i$ (p. 104)—for that is what it amounts to—with 31 entries instead of 54 and with 23 blanks. He proposed to fill

[6]He treats it more thoroughly than we do, stating inequalities that must be satisfied.

[7]The question is most decidedly not rhetorical; I am puzzled. I lay great weight on the statement of the author (p. 84) relative to the apparent convergence of a great variety of initial assumptions to the same final table of values. If I had proceeded as he did and found the same appearances I should have drawn his conclusion so far as I can now see. Yet in view of the general situation which I am discussing, I should equally well ask myself how I got the unique result, what tacit assumptions had been introduced and what their significance? Apparently he is still working on the problem, asking himself such questions.

in 31 values of the intensities and leave 23 blank. Now can he really do it? This would seem to amount to setting 23 of our 54 unknowns equal to 0, leaving us 36 equations to solve for 31 unknowns. This cannot be done without 5 supplementary equations among the $r_{ij}$. It looks to me as though his method of reduction must have resulted in introducing such equations or in being satisfied with an approximate solution (as of course he had to be by his arithmetical method of successive approximations), of which the actual non-existence was covered by sampling errors or neglected decimal places. In his second example (p. 122) there are $N=13$ tests and $G=7$ traits leading to 91 unknowns and 78 coefficients of correlation. He has 29 blanks in his table. It looks to me, if my count is sound, as though he were entitled to 13 blanks for a unique solution—13 arbitrary blanks. But how many of the additional intensities would be practically 0, I do not know. We may observe that he has no entries of values between $-.062$ and $+.054$ but five values between $-.062$ and $-1.0$ (inclusive) and eight entries between .054 and 1.0 (inclusive). If we allow one entry for each .007 between —.062 and .054, which is about the average density between $-.062$ and $-1.0$ and .054 and 1.0, we should place 15 or 16 entries in this blank region and with 13 arbitrary blanks should have about the total of 29 blanks which occur in the table. In his first sample he has no entries between $-.09$ and $+.09$ and we might consider that he had 18 actual blanks to which he was entitled and 5 values which lay between the limits of $\pm.09$. But this is only a guess.[8]

The fact of the matter is that the author after an excellent introductory discussion of the elements of the theory of the resolution into general and specific factors (pp. 34-65), gives up the general problem entirely, throws up his hands so to speak, and proceeds to develop special methods of examining his variables to see where there seem to be special "bonds" between them (pp. 66-96) which might indicate those places in his table of $NG$ intensities which should differ markedly from zero and those which should not, and to developing arithmetical methods of finding by least squares through successive approximations such values for those intensities which he decides are not practically zero as will best reproduce the values $r_{ij}$ of the given correlation coefficients.

I do not object to this abandoning of the general analysis for a special procedure. In a new line of work any way forward is justifiable. He gets a good fit—the worst reproductions differ from the given values by less than their probable error. For inferential purposes may not such a fit,

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[8]I should also guess that one could ordinarily find by least squares an excellent fit to the 36 values $r_{ij}$ for $N=9$ with 5 general factors instead of 6. For with 5 we have 45 intensities to determine with $5 \times 4/2=10$ of them more or less arbitrary, due to the orthogonal transformation, and we have reason to believe that a least-squares solution of 36 equations for $45-10=35$ essential unknowns would in general yield a first-class fit unless the 36 values of $r_{ij}$ happened to be such as to make the solution imaginary. The author's solution with $G=6$ is imaginary anyhow; maybe he really needs more than 6 general factors to get it real. It is noticeable that the most critical $r$ is $r_{56}=.96$ (corrected) or .72 (raw) for which the author gets .67 from his approximate solution or about .91 corrected. I do not see anything in the table that would make the solution imaginary for such a value of $r_{56}$, though there may be something that does, and if there is we may have to use more general factors.

as a fit, be better than is justifiable rather than worse? It is, however, useful I think to distinguish between logical uniqueness under general considerations and a statistically satisfactory fit by least squares. A really practical question is: How many blanks can in any case be left and get a fit that is satisfactory? Another practical question if one wishes to place some dependence on the significance on the final values of the intensities is: How much difference does it make which spaces are left blank? How much does what you want to get determine what you do get over and above that to which you are really entitled? Must we refrain from postulating certain spaces as blank if we are to avoid clashing with our inequalities which limit the solution to real values, and does herein lie the real significance of Kelley's method of searching for special "bonds" between the variables?

It is interesting to apply our equations to the example on p. 140. Here $N=10$ and $G=7$. Now our equation for $G$ gives

$$G=\tfrac{1}{2}[21-\sqrt{81}]=6$$

Thus, if our equation is correct, just 6 factors will give a unique (though individually indeterminate) solution except for the 15 degrees of freedom due to the orthogonal transformation. The author uses 7 factors—one more than appears necessary. He has then 70 intensities and 45 equations given by $r_{ij}$. Even allowing for the $\infty^{21}$ orthogonal transformations there are 4 degrees of freedom; without allowing for them there are 25. He has 30 blanks in his table. He is apparently entitled to 25 and doubtless five more might come along statistically. But if our equations are correct he can use 6 general factors, have only 60 intensities to determine from the 45 equations, and have a unique exact solution with just 15 blanks in his table without appealing to least squares. Or he can postulate more than 15 blanks and solve by least squares to get the best fit he can.

4. *Verbal.* A fundamental question which I can hardly know enough to ask aright is: What are these general factors, Verbal, Speed, Spatial 2, etc.? Are they like Spearman's $g$— intelligence—something intrinsically defined by the analysis? Recall that he says that he does not define $g$ but measures it (and, I believe, attempts to interpret it). Or are these general factors certain known traits definable otherwise than by our analysis? And is our problem really to explain the observed correlation coefficients in terms of these known traits? If the problem is this, we do not need any discussion of the general problem of resolution of variables into unknown general and unknown specific factors. We can proceed at once to select our known general factors and see how well we can reproduce the observed correlations by regarding only the intensities as unknown and fitting by least squares (we merely have to have fewer intensities than correlation coefficients). Naturally a preliminary discussion might well somewhat mix the two points of view unconsciously, striving merely to attain a sensible end.

Kelley seems to doubt the existence of Spearman's general factor $g$— the general intelligence. He says (p. 110) that the analysis of the heterogeneity factor $a$ (maturity, race, sex, and general nurture) might lead us to wonder whether if we had experimentally allowed for these four elements any general factor $g$ would have remained. How about genetic constitution, that with which we start as a fertilized egg? Is this all contained in race, sex, maturity, general nurture, verbal, number, memory, spatial 1, spatial 2, speed, vivacity? We have a great many chromosomes and an unknown number of genes. How many traits are needed to take care of genetic constitution? And how about those factors which the common person thinks of as general characteristics of the individual, such as

reliability, attentiveness, docility, originality, which do not explicitly appear? Are they adequately covered by spatial 1 and spatial 2, by speed and memory, by verbal and vivacity, etc.?

What is the significance of group traits? Do they interest the teacher or only the educational administrator or only the scientific investigator? What is even the significance of the resolution into the general and the specific? In my review of Spearman's book I ventured the suggestion that possibly the explanation of the general factor should have laid more emphasis on the tests and less on the general intelligence. Kelley certainly does not emphasize the latter. His point of view is, I think, contained very briefly in his statement on p. 23: "Mental life does not operate in a plain but in a network of canals. . . .though each mental trait may grow and become more subtle, it does not lose its character and discreteness from other traits." Bearing on his point of view is the author's discussion of table XXVIII (p. 135), I am not enough of a psychologist to understand, to say nothing of passing judgment on this.

As an enumerator I will point out the interesting fact that in the combined table for 3rd-grade and 7th-grade pupils with nine tests and six general factors there are just 18 blanks in common. There are 36 intercorrelations and 54 intensities. If, then, our count is correct, the author is entitled to a unique solution with just those 18 blanks except as he may run into clashes with his inequalities. Note that there are 23 postulated blanks in the 7th-grade table and 24 in the 3rd-grade table overlapping in 18 places but not in 6 for the 7th-grade and 5 for the 3rd-grade. It would be interesting to postulate the 18 common blanks and get the exact solution (if such there be) for each of the two grades, noticing particularly if the results bore the same final interpretation.

It is noticeable to me, though it may be without real significance, that for the 3rd grade we have an intensity of −.32 for Verbal in Memory-Meaningless Symbols corresponding to a postulated 0 for the 7th-grade, whereas we have an intensity of .46 for Memory in the same test in the 7th-grade corresponding to a postulated 0 in the 3rd-grade. Is the "bond" theory and analysis sufficiently trustworthy to let us be confident that our postulates are sound? I should think that we needed to make this little arithmetic investigation of postulating zeros in only the 18 common blanks before we embarked upon the interesting psychological inferences and suggestions of p. 136. Also this much seems fairly clear to me, although the author might not agree with me, viz.: Possibly the explanation of the general factors should have laid more emphasis on the tests and less on the general traits.

When a professional journal asks a person not professional in its line to review a professional book that is highly technical and busied with the newest research of a leader in his field, it can hardly expect to receive a real review but only a discussion of some aspects of the work that may touch upon the special abilities or attainments of the reviewer without drawing too heavily upon his perhaps non-existent general intelligence. Indeed the perusal of the great variety of new research literature which has passed before my eyes in the last 30 years leaves me impressed with its highly technical appeal and its lack of appeal to the general intelligence. This is as it should be if there is no such thing, and as it must be even if there is such a thing. I am still satisfied with a master's book as he writes it, be he Kelley or Spearman.

5. *Spatial.* I cannot stop without again referring to the orthogonal transformation. In Kelley's first example we have (p. 104) for the resolution (omitting the specific parts):

| | |
|---|---|
| Reading-Speed | $=x_1=.40\alpha+.69\beta+0\ \gamma+0\ \delta+.09\epsilon+.38\zeta$ |
| Reading-Power | $=x_2=.38\alpha+.70\beta+0\ \gamma+.20\delta+0\ \epsilon+0\ \zeta$ |
| Arithmetic-Speed | $=x_3=.23\alpha-.15\beta+.60\gamma+.17\delta+0\ \epsilon+.66\zeta$ |
| Arithmetic-Power | $=x_4=.21\alpha+0\ \beta+.63\gamma+0\ \delta+.31\epsilon+0\ \zeta$ |
| Memory-Words | $=x_5=.66\alpha-.09\beta+0\ \gamma+.56\delta+0\ \epsilon+0\ \zeta$ |
| Memory-Numbers | $=x_6=.62\alpha+0\ \beta+.25\gamma+.46\delta+0\ \epsilon+0\ \zeta$ |
| Memory-Symbols 1 | $=x_7=.59\alpha+0\ \beta+0\ \gamma+.52\delta+36\epsilon+0\ \zeta$ |
| Memory-Symbols 2 | $=x_8=.39\alpha+0\ \beta+0\ \gamma+.46\delta+.64\epsilon+0\ \zeta$ |
| Space-Power | $=x_9=.65\alpha+0\ \beta+.14\gamma+0\ \delta+.48\epsilon-31\zeta$ |

Here $\alpha$ is Heterogeneity, $\beta$ is Verbal, etc. It is not surprising that in reading, the verbal factor should be strong, or in arithmetic the number factor. But what I do not understand is where these factors and their intensities really come from. They certainly do not come merely from the resolution into the general and specific, as a consideration of the orthogonal transformation will show.

The general rotation in hyperspace of six dimensions contains 15 parameters. Much has been written by mathematicians, including some by me, on these multi-dimensional rotations. I could give general formulae for the case but will content myself with sticking to two factors—let us say $\alpha$ and $\beta$. We have

$$\alpha=\alpha' \cos q-\beta' \sin q,\ \beta=\alpha' \sin q+\beta' \cos q$$

Then substituting, we find

$$\begin{aligned}
x_1&=(.40\cos q+.69\sin q)\ \alpha'+(.69\cos q-.40\sin q)\ \beta'+\ \ldots\\
x_2&=(.38\cos q+.70\sin q)\ \alpha'+(.70\cos q-.38\sin q)\ \beta'+\ \ldots\\
x_3&=(.23\cos q-.15\sin q)\ \alpha'+(.15\cos q-.23\sin q)\ \beta'+\ \ldots\\
&\ldots\ldots\ldots

as to any other and so far as any information that can be extracted from the intercorrelations goes any one of the tables is entirely equivalent to any other—there is no way to choose between the $\infty^{15}$ tables unless we use something more than is contained in our 36 coefficients $r_{ij}$, and any interpretation that is put on any of the $\infty^{15}$ tables should be independent of which one of the $\infty^{15}$ we take!

How does this bear on the canal theory of traits? I do not know. Mathematically the theory of resolution into general and specifics gives no indication of canals, and mathematically Kelley's statement (p. 23) should read just reversed, viz.: Mental life in respect to its resolution in general and specific factors does not operate in a network of canals but in a continuously distributed hyperplane. . .no trait has any separate or discrete existence, for each may be replaced by proper linear combinations of the others proceeding by infinitesimal gradations and it is only the complex of all that has reality. Instead of writing of crossroads in the mind of man one should speak of man's trackless mental forest or tundra or jungle—mathematically, mind you, according to the analysis offered. The canals or crossroads have been put in without any indication, so far as I can see, that they have been put in any other way than we put in roads across our midwestern plains, namely, to meet the convenience or suit the fancy of the pioneers.

On the other hand if mental life does proceed in canals that are discrete I see not the slightest reason for supposing that the general traits will be uncorrelated and plenty of reason to believe that they will not be. Linkage is quite too well-known a phenomenon in genetics, too general a phenomenon, to be overlooked. Our physical stature does not appear to run in canals. If we could go down to the detailed genetic constitution of each individual, we might get variables (genes) in terms of which we could use discreteness as a fundamental idea in mapping out each of us as he is, whether physically or mentally, but we are not likely to discover that structure by the statistical analysis of our various characteristics, height, weight, vital capacity, eye-color, baldness, blood-pressure, arithmetic ability, spatial perception, verbosity, etc. Furthermore, there is always the possibility that when we know better than even T. H. Morgan now does the physical bases of heredity we shall be less confident believers in original sin or genetic constitution and be more ready to credit some fundamental as well as superficial effects to nurture.

This is all speculation and may be quite beside the point or even largely wrong; but mathematics is mathematics with both its limitations and its advantages. Among its limitations is this one: You do not get anymore out of it than you put in, although you may get it out in much more useful form than it went in. Among its advantages is this: It shows you what you have left out and keeps open the door for additional entries. You cannot get discreteness of the general factors out of the resolution into general and specific; you cannot get uniqueness where there is none. If there is one general factor only its total or group intensity is unique in each test (but its value for the persons tested is indeterminate); if there are two or more general factors their intensities are not unique but the sum of the squares of those intensities is unique in each test; from an examination of the intercorrelations alone you cannot determine "bonds" between the variables which will suffice to determine what blanks must be left in the table of *NG* intensities but only those recondite connections which are common to the whole group of possible tables?[9] So much follows from geometry of hyper-

[9]I would not say that the examination might not indicate which spaces might be left blank, but if they are filled by some orthogonal transformation

space. I have an idea that their tools are in some spots sharper than Kelley and Spearman imply. I hope they do not hurt themselves—it's such fun, and withal so useful, to whittle.

6. *Speed.* In reviewing Kelley's book on statistics some years ago I commented on the care with which he estimated his probable or sampling errors; he points out here that Spearman's samples are small. In my review of Spearman I mentioned that the statistical instability of the answer was great and I might go a bit further into the matter. If there are three tests only, the resolution into general and specific is

$$x_1=\sqrt{\frac{r_{13}r_{12}}{r_{23}}}\,g+e_1,\qquad x_2=\sqrt{\frac{r_{23}r_{12}}{r_{13}}}\,g+e_2,\qquad x_3=\sqrt{\frac{r_{13}r_{23}}{r_{12}}}+e_3$$

The coefficients of $g$ are the intensities with which the general factor enters into the variables. What is the sampling or other probable error of this intensity?

$$c_1=\sqrt{\frac{r_{12}r_{13}}{r_{23}}}\qquad \log c_1=\tfrac{1}{2}(\log r_{12}+\log r_{13}-\log r_{23})$$

$$\frac{\delta c_1}{c_1}=\tfrac{1}{2}\left(\frac{\delta r_{12}}{r_{12}}+\frac{\delta r_{13}}{r_{13}}-\frac{\delta r_{23}}{r_{23}}\right)$$

We note the relative variation, $\delta c_1/c_1$, in $c_1$ is one-half the sum or difference of the relative variations of the intercorrelation coefficients. But the sampling error of $r$ is, except when $r$ is large, practically an absolute quantity, $1/\sqrt{n}$. Without trying to get an exact expression for $\delta c/c$, which would involve the correlations between the correlation coefficients we may estimate that the relative standard deviation $\sigma_c/c$ is about $1/(2r\sqrt{n})$ when $r$ is the smallest of the three intercorrelations involved. Kelley's population in the 7th grade is 140 (alternatively 109); as an average, one may take $\sqrt{n}=11$, so that, if we attempted to resolve a group of three of his tests into their general and their specific factors, the relative error of the intensities would be roughly $\sigma_c/c=1/(22\ r)$ where $r$ was the least intercorrelation of the three. No value of $c$ would then be really significant if any $r$ involved were less than 0.1, and as we have underestimated rather than overestimated the value of $\sigma_c/c$, it might well be that the statistical indetermination would extend to values of $r$ nearly up to .15. Consider the tests 2, 4, 5 with $r_{24}=.0655$, $r_{25}=.3097$, $r_{45}=.1487$. The formal resolution would be

$$x_2=.36g+e_2,\quad x_4=.17g+e_4,\quad x_5=.8g+e_5,$$

and one might conclude that $x_5$ contained a great deal of $g$, but it seems to me doubtful whether we know anything about the amount of $g$ in any of the tests (because $\sigma_c/c$ seems to be pretty much the same for all three intensities as is natural when all three involve each value of $r$, of which one is scarcely significant).

What the sampling errors of the intensities in Kelley's resolution of the 9 tests into 6 general factors may be I do not know, but that they may well be

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the interpretation of them as blanks is not justified on the basis of the correlation coefficients.

so large as to make difficult any inferences from the comparative table (p. 135) of 3rd-grade and 7th-grade pupils is at least conceivable, and if the same sort of thing as was true for three tests and one general is true for nine tests and six generals, namely, that some of the large intensities may have about the same relative errors as some of the small ones (which nobody would think were significant) the comparison is indeed precarious. It is only fair, however, to point out that his resolutions of the tests for 140 and for 109 7th-grade pupils (pp 104, 122) show a remarkable stability in the larger coefficients (the value .855 in Table XXI for $6_a$ is obviously a misprint). Yet another consideration may be advanced: Table X for the intercorrelations taken with the reliability coefficients is inconsistent, as the author remarks, and his resolution is imaginary. The imaginary entry is $.17\sqrt{-1}$. This would indicate to me that a sampling error of about the magnitude of .17 is involved in this place in the resolution. And further: if the table be modified slightly so as to become consistent I believe that in passing from the imaginary resolution to a real resolution we should find intervening a degenerate resolution, i.e., one in which the nine tests were linearly dependent and one might find that three particular tests became linearly dependent (as would be the case if the corrected coefficient $r_{56}=.9596$ were changed to .9563 which would be equivalent to changing the raw $r_{56}$ from .7169 to .7147 and leaving the reliability coefficients alone—in which case tests 4, 5, 6 become linearly dependent). Now if three tests are linearly dependent so that $x_i=\alpha x_j+\beta x_k$ for some specified values of $i, j, k$ it is impossible that there be any specific factor in $x_i$ or $x_j$ or $x_k$ because if $e_j$ and $e_k$ be specific factors in $x_j$ and $x_k$ we have $e_i=\alpha e_j+\beta e_k$ and the condition that $e_i$ and $e_j$ and $e_k$ be all independent requires $e_j=e_k=e_i=0$. Furthermore, if instead of having the linear dependence among three of the variables, it is found among all nine of them, but not among any fewer number, so that

$$x_9=a_1x_1+a_2x_2+a_3x_3+a_4x_4+a_5x_5+a_6x_6+a_7x_7a+a_8x_8,$$

with none of the values of $a$ vanishing, then

$$e_{19}=a_1e_1+a_2e_2+a_3e_3+a_4e_4+a_5e_5+a_6e_6+a_7e_7+a_8e_8.$$

Now the condition that $e_9$ be specific, i. e., uncorrelated with $e_1, e_2, \ldots, e_8$ requires that all nine of the specific factors $e$ shall vanish.

We come, therefore, to a general theorem in the theory of the resolution of a series of variables into any number of general factors plus factors specific to the variables, viz.: In case any of the variables are linearly dependent with non-vanishing coefficients, then none of that group can contain specific factors. Now it may be that if the sampling errors were nonexistent and we had the true and perfect values of the correlation and reliability coefficients, such a situation would never arise; but, data being what they are, the situation does arise and arises frequently that some set of the variables is linearly dependent within the sampling errors. This means fewer specific factors and, I fear, more general factors. Take now the tests 4, 5, 6 which are inconsistent and suppose the modification which makes them linearly dependent, i. e., a change of $r_{56}$ corrected from .9596 to .9563 or from .7169 to .7147 (raw). We must have linear dependence between $x_4$, $x_5$, $x_6$,

and in fact I find $x_6 = .304\, x_4 + .891\, x_5$. The author gives

$$x_4 = .21a + 0\ \beta + .63\gamma + 0\ \delta + .31\epsilon + 0\ \zeta + .16e_4$$
$$x_5 = .66a - .09\beta + 0\ \gamma + .56\delta + 0\ \epsilon + 0\ \zeta + .36e_5$$
$$x_6 = .62a + 0\ \beta + .25\gamma + .46\delta + 0\ \epsilon + 0\ \zeta + .17\sqrt{-1}e_6$$
$$.304x_4 + .891x_5 = .65a - .08\beta + .19\gamma + .50\delta + .09\epsilon + 0\ \zeta + .05e_4 + .33e_5$$

On comparing the last two lines we find that differences of from $-.08$ to $+.09$ occur in the coefficients of the general factors and that the differences in the intensities of the specific factors run from $.17\sqrt{-1}$ to .33. As the change in $r_{56}$ is quite small, much less than the sampling error, we get an idea of the possible size of discrepancies in the intensities, including of course any approximations of Kelley's solution.[10]

7. *Vivacity.* If it can be thoroughly understood that I am perfectly willing and even anxious to have this theory of the general and specific urged forward to a more completed form so that we may better judge of its value and offer my own questions and suggestions in good faith, not for rhetorical effect or to discourage, I will venture to suggest that useful as the resolution may be for a number of purposes it is not likely to prove of any very serious permanent general scientific significance. And why? Consider three tests: They cannot possibly depend upon a single real trait or character except for their specific significance, because, indeed, if we add a fourth test we have to change that general trait. That is to say, given $x_1$, $x_2$, $x_3$ with certain inequalities satisfied, then

$$x_1 = c_1 g_1 + e_1, \quad x_2 = c_2 g + e_2, \quad x_3 = c_3 g + e_3$$

is unique with respect to the intensities $c_1$, $c_2$, $c_3$ with which $g$ enters into $x_1$, $x_2$, $x_3$; but if we adjoin $x_4$, although we may get a resolution into two generals in an infinite number of ways as

$$x_1 = c'_1 g' + k_1 h + e'_1 \quad x_2 = c'_2 g' + k_2 h + e'_2$$
$$x_3 = c'_3 g' + k_3 h + e'_3 \quad x_4 = c'_4 g' + k_4 h + e'_4$$

we cannot maintain the same general $g$ but must take another $g'$. The reason we cannot maintain it is because $r_{1g} = c_1$ whereas $r_{1g'} = c'_1$. If then $g$ and $g'$ were the same, $c_1$ and $c'_1$ would be the same and $k_1$ would be zero so that the first variable would be $x_1 = c_1 g + e_1$ as before. But then so would the other two variables $x_2$ and $x_3$ remain as before. And then $x_4 = c_4 g + k_4 h + e_4$ would give $r_{14} = c_1 c_4$, $r_{24} = c_2 c_4$, $r_{34} = c_3 c_4$ which are quite too many conditions on $c_4$. Hence, in general, the adjunction of another test $x_4$ changes the general traits of those tested—the general trait $g$ is relative to the three tests, it is not a characteristic of the group tested except relative to those tests. In the presence of the information furnished by another test there are two traits, $g'$, $h'$, necessary and the previous trait $g$ is not contained among them unless certain restrictive conditions are satisfied.

The proof above given is not water-tight. It is not necessary that $k_1$ be zero provided $e'_1$ is different from $e_1$, but why should that which is specific to test 1 change when we adjoin another test? And suppose it did change so that

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[10] The linear relation among $x_6$, $x_5$, $x_4$ depends on the use of the corrected coefficients, but due to the sampling errors of the reliability coefficients the relation among the three variables is not strictly linear because $r_{6i}$ (corrected) is not strictly equal to $.340r_{4i} + .891r_{5i}$ for $i$ other than 4 or 5. I think, however, that the argument I use will do to throw some light on the presumptive magnitudes of the sampling errors in some of the intensities.

$$x_1 = c_1 g + e_1 = c_1 g + k_1 h + e'_1$$
$$x_2 = c_2 g + e_2 = c_2 g + k_2 h + e'_2$$
$$x_3 = c_3 g + e_3 = c_3 g + k_3 h + e'_3$$
$$x_4 = c_4 g + k_4 h + e'_4$$

and that which was specific to the three tests now contains the new general $h$. We should then have

$$r_{12} = c_1 c_2 = c_1 c_2 + k_1 k_2 \qquad \therefore k_1 k_2 = 0$$
$$r_{13} = c_1 c_3 = c_1 c_3 + k_1 k_3 \qquad \therefore k_1 k_3 = 0$$
$$r_{23} = c_2 c_3 = c_2 c_3 + k_2 k_3 \qquad \therefore k_2 k_3 = 0$$
$$r_{14} = c_1 c_4 + k_1 k_4, \quad r_{24} = c_2 c_4 + k_2 k_4, \quad r_{34} = c_3 c_4 + k_3 k_4$$

Now as two of the three quantities $k_1$, $k_2$, $k_3$ must vanish we still have two of the three equations $r_{14} = c_1 c_4$, $r_{24} = c_2 c_4$, $r_{34} = c_3 c_4$ to determine the one constant $c_4$, and this is too many. I think that makes the proof quite watertight.[11]

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[11] If $k_1 = k_2 = 0$, $r_{14} r_{23} = r_{24} r_{13}$ and one of the tetrad equations vanishes. Then $k_3$ need not vanish, the term $k_3 h$ may come out of $e_3$, and $g$ as defined by 1, 2, 3 remains unchanged by the adjunction of $x_4$, and the other two tetrad differences $r_{14} r_{23} - r_{12} r_{34}$, $r_{24} r_{13} - r_{12} r_{34}$ become numerically equal to each other, being—$c_1 c_2 k_3 k_4$. Kelley makes fundamental use of these facts in looking for "bonds"; in fact he shows that if one of the tetrad differences vanishes, the resolutions of four variables may be so made that one general runs through all four and the other through only two of the four variables. Owing, however, to the possiblity of the orthogonal transformation upon the two generals there is no assurance that the generals will appear in the simple distribution in which they may appear. Of course, if the generals are defined by the analysis, and in no other way, then any special resolution which is effected by any particular investigator becomes his definition of the generals; but as there is nothing general about that resolution, the generals are relative to him.

If one should insist that $e_3$ is really specific to $x_3$ and in no way depends on $x_4$, we must have $k_3$ also equal to zero and all three tetrad equations will vanish. This is Spearman's case; $c_4$ is then determined, but as $k_4$ is arbitrary we need not assume that there is no general part $k_4 h$ in $x_4$ but may divide $c_4$ with $k_4 h + e^1_4$ to suit ourselves.

In my articles previously cited, I showed that if the variables $x_1$, $x_2$, . . . are replaced by linear combinations $x^1_1$, $x^1_2$, . . . of themselves the tetrad equations will in general not continue to be satisfied and that even if they still are satisfied, the $g^1$ may differ markedly from $g$, so that the concept of $g$ is generally not preserved. In the discussion just given, it is shown that if $x_1$, $x_2$, $x_3$, are preserved and $x_4$ is added, the resolution of the four into two generals cannot ordinarily preserve that which was general to $x_1$, $x_2$, $x_3$. When Kelley (p. 122) resolves 13 variables into 7 factors and uses the same symbols and names for 6 of these generals as in his resolution of 9 variables into 6 factors (p. 104), he must, so far as I can see, change the significance to be attached to the 6 generals even if he changes neither the symbols nor the names, unless certain equations between the correlation-coefficients are satisfied; and, so far as I can see, he has not exhibited the equations. The remarkable statistics of the intensity coefficients in corresponding places in the tables of pp. 104 and 122 may mean that the meaning of the words have not much changed, or, better, that they need not be much changed (for after all, the generals are so largely indeterminate as was Spearman's $g$ that entirely apart from the orthogonal transformation it is a question as to whether they specify concepts at all).

Thus the traits Heterogeneity, Verbal, Number, Spatial, etc., are relative to the tests and the adjunction of new tests will, except for restrictive conditions, make it necessary to change the significance of Verbal, Number, Spatial, etc., including Heterogeneity. The words mean different things for the same group of individuals if we leave the tests which define them alone and adjoin new tests. To my way of thinking this is fatal to the whole theory unless we mean by the theory only a convenient analysis of a specified group of tests. However, there may be a great deal of use in having such an analysis of a given group of tests. Our educational system is not quite infinitely varied, our tests whether psychological or otherwise may well run in ruts and those ruts may well be worth a great deal of careful examination. Some of us suspect that education does not proceed upon a plane but in a series of canals and that the canals are not adequate to contain the flow of the mind of our youth, and we should like to know more about the canals and in particular where we might best build new ones.

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COMMENTS UPON EDWIN B. WILSON'S REVIEW OF *Crossroads in the Mind of Man: A Study of Differentiable Mental Abilities.*

It is a privilege and a pleasure to add a few comments to Dr. Wilson's illuminating review of my study of differentiable abilities. I shall deal with broad issues after first noting a few matters of detail.

As surmised by Wilson, the entry .855 in the first column of Table XXI, p. 122, opposite "Memory-Numbers is in error. This entry should be .585.

I fully agree with Wilson that my proposition 15, p. 58, is in error. The equality of the pentad criterion to zero is a necessary condition, but not as stated a sufficient condition, that five variables may be thought of as due to two general factors plus five specific factors.

Dr. Wilson raises the question as to whether the goodness of fit which I get between reconstructed correlation coefficients built upon the factors which I derive and the actual correlation coefficients is not "for inferential purposes . . . better than is justifiable rather than worse." I believe it is very considerably better than is justifiable. An a priori theory yielding theoretical coefficients can be tested against reality by comparing the theoretical with the actual coefficients. We may say, at least approximately, that if the median difference between the theoretical and actual coefficients is just equal to the median probable error of the actual coefficients, then the theory just does adequately account for the observed facts. If this median difference is greater than the median probable error then the indication is that the theory is inadequate, while if it is less the theory is more adequate than we have a right to expect. Now the factor value that I derived were not due to a priori considerations but were derived by an indirect and long process from the observed correlation coefficients themselves. Accordingly, when a fit better than we have a right to expect is obtained, the reasonable conclusion to draw is that one has employed too many factor values, thus capitalizing chance variations. The agreement then is spuriously high as a picture of the truth. In my Table XIII, p. 106, are 36 correlation coefficients having a median probable error of .05. The median difference between these coefficients and the theoretical coefficients is .012, thus indicating too good a fit to be reasonable.

Let us be given results for a nine-variable problem, yielding 36 inter-

correlation coefficients, the median probable error of which is .05, as indicated in Table A herewith. The first column gives the number of factor values assumed in the theoretical lay-out while the second column gives the median difference in absolute value between the observed and the theoretical coefficients. Except for the tenth row, which gives the actual results found in my study, the entries in the second column are hypothetical, though in the case of the first row the value assigned is not entirely hypothetical, for investigation showed that the median difference would be very much greater than .05.

TABLE A

(Largely Hypothetical)

THE AGREEMENT BETWEEN CORRELATION COEFFICIENTS BUILT UPON VARIOUS THEORIES AND OBSERVED COEFFICIENTS, IN THE CASE OF A CERTAIN NINE-VARIABLE PROBLEM, THE MEDIAN PROBABLE ERROR OF THE COEFFICIENTS OF WHICH IS .05

| Hypothesis | Median difference between reconstructed $r$'s and observed $r$'s |
|---|---|
| 1:1 general factor, requiring 9 factor values | .15 |
| 2:2 factors, requiring $9+8=17$ factor values | .10 |
| 3:3 " " $9+5+3=17$ " " | .07 |
| 4:3 " " $a+b+c=17$ " " | .065 |
| 5:4 " " $a+b+c+d=17$ " " | .05 |
| 6:5 " " $a+b+c+d+e=17$ " " | .06 |
| (No one of $a, b, c, d, e,$ equals 0) | |
| 7:3 factors, requiring $9+8+7=24$ " " | .06 |
| 8:4 " " $9+8+7+6=30$ " " | .03 |
| 9:5 " " $9+8+7+6+5=35$ " " | .001 |
| 10:6 " " $9+6+5+4+4+3=31$ " " | .012 |
| 11:6 " " $9+8+7+6+4+2=36$ " " | .0000 |
| 12:$x$ (greater than 5) factors requiring a total of 36 factor values | .0000 |

Table A is, I believe, roughly descriptive of the facts as embodied in the data for my population of 140. I am assuming with Wilson, as I have throughout my entire work, that I would get a perfect fit (though some of my factor values might be imaginary) when utilizing the hypotheses of rows 11 and 12, so that in these cases the entry in column 2 is exactly zero. Dr. Wilson seems somewhat uncertain as to his count of the degrees of freedom in the system. I judge that I did not know enough to feel uncertain in this situation. At any rate I have used a method exactly equivalent to his. As my argument has only required that the number of parameters in my theoretical system be less than the number of degrees of freedom, I trust that I have always been upon the safe side.

Dr. Wilson explains many of the important orthogonal and other relationships holding in such a system, as indicated in row 11, and I believe he would agree likewise in row 12. With his explanations I feel in perfect accord. In a paper given before the American Psychological Association, Cambridge, December 1922, I showed the infinite possibility of interpreting mental organization by orthogonal transformations if as many as two general

factors are present, and I briefly mentioned the still greater possibility in case of a larger number of factors. I have long been familiar with this point of view and will even say that it is basic to my present view. It is basic in the sense that somewhere in the process a condition must be imposed favoring some one solution rather than any other one of the infinite number of orthogonal equivalents.

If at this point we note Wilson's orthogonal substitution in his paragraph 5, we will see that whereas in my solution there are nine $\alpha$ factor values and four $\beta$ values other than zero, or a total of 13, there are, after making the transformation, nine $\alpha'$ values and nine $\beta'$ values, or a total of 18. By the principle of parsimony my solution is better than his. This principle is just about the only guide that I can find in this unchartered realm. It changes "man's trackless mental forest" into a land seamed with "crossroads."

If Spearman's single general factor theory were adequate to explain phenomena—in other words if row 1 of my Table A above had the value .05 in the second column—then I and, I believe, all people would choose to explain the phenomena in these simple terms. If the simple picture is insufficient we will resort to a larger number of factors, or of bonds, but only to as large a number as are demanded by the data, which should be very comprehensive so as to tap all phases of mental make-up. Having determined this larger number of factors—two, three, or more as the case may be—then the question of prime importance is, "Does one orthogonal transformation serve as well as another?" I am confident the answer is "no," but at this point I admit being in a "groping stage." I hope it is "progressive." The groping, however, is in a search for a method to determine the one of the possible systems which is most useful and not a groping due to a failure to realize the mathematical equivalence of the systems.

Referring to Table A, I am searching for the solution given by row 5, not by row 10 and certainly not by rows 11 or 12. The solution I found (row 10) has, like row 8, gone too far. I should have indicated this clearly in my treatise in order that the goal of the undertaking be not misleading. If I get a solution, as row 5, such that no solution having fewer parameters yields as good a fit and such that no orthogonal transformation of the row 5 solution has as few parameters, then I will consider that I have "the" solution. I would better make a reservation here: If a solution involving 3 factors and 17 parameters yields a median discrepancy of .05 and if a solution involving 4 factors and 17 parameters does likewise, I would be at a loss to choose between them. Perhaps the probable errors of these parameter values, if they can ever be obtained, will throw light upon this matter.

Now where in my process have I imposed any condition that prejudices my solution in favor of the row 5 type rather than of an orthogonal transformation of it? This has been done, I hope and am inclined to believe, in limiting the number of cells filled, i.e., the number of factor values other than zero, to a number less than the number of degrees of freedom. I hope this point may be settled by mathematical analysis, or, lacking that, by repeated experimental findings.

Whether my procedure gives a solution or not, it seems logically necessary that there must, in general, be a solution. Stated in general terms: given $N(N-1)/2$ intercorrelations of $N$ variables and a table of $N$ variables and $M$ factors ($M$ may be as large as one chooses), then, if but $F$ [$F$ less than $N(N-1)/2$] factor values are to be assigned real values other than zero, there is one particular set of $F$ cells which should contain these $F$ values in order to give the best fit. (Always, of course, note that interchanging any two of the $M$ columns in the table does not change the system.)

Assuming the original population large and representative and the $N$ variables such as to tap fairly completely the entire mental field, then the picture of mental structure provided by the $F$ entries is the most accurate picture which it is possible to provide with $F$ elements. If, further, $F$ is the minimal number that will suffice to describe the situation adequately, then the picture of mental structure is economical of mental concepts. A minimum number of relationships need to be pictured and all of these may be expressed in terms of independent traits. There are probably other derivative advantages.

To make the picture entirely adequate and serviceable in the handling of practical psychological problems, separate measures of the independent traits should be secured. Upon this point I do not share the skepticism of Wilson, who has pointed out that I do not assign trait values to the successive individuals in my population, just as Spearman did not give individual $g$ values. It is, of course, true that a $g$ factor (or, if that is inadequate, $\alpha$, $\beta$, etc., factors) as determined from a small number of tests upon a certain population may not be the same as a $g$ (or $\alpha$, $\beta$, etc.) factor as determined from an expanded or a second series of tests. If, however, the number of tests is great and if they quite completely sample all mental capacities the situation is quite different. A $g$ (or $\alpha$, $\beta$, etc.) as now determined will be a stable thing for the individual. A second set of tests, different but equally comprehensive, will yield the same individual $g$ (or $\alpha$, $\beta$, etc.) values.

In short, I look upon these factors as real traits "definable by our analysis" in the first instance but capable of isolation experimentally by means of specially devised tests, or algebraically by means of linear combinations of such tests as have been used—these combinations being such as to cancel all factors except one.

I have used the same terms to describe factors as derived from different populations and from the same populations, employing in the second instance an expanded number of tests. As noted by Wilson, "the words mean different things for the same group of individuals if we leave the tests which define them alone and adjoin new tests." I cannot defend my procedure logically though I believe in the case in question little practical error is involved. In fact, I believe a use of different words would have unduly complicated the situation. For example, the "Verbal factor" is defined in the first instance by its presence in four of the nine tests employed as follows: reading-speed .69, reading-power .70, arithmetic-speed −.15, and memory-words −.09. The same term is used in the second instance for a factor present in six of thirteen tests: reading-speed .68, reading-power .73, arithmetic-speed −.08, memory-numbers −.07, intellectual interest .42, and social interest .42. The last two tests were not in the battery of nine. To use two terms for these slightly different functions would seem uncalled for while the issues are still as crude as at present.

In such a study as that of mental factors many things may be mathematically unequal that are for practical purposes quite equal. As I see it, the problem calls for statistical sufficiency rather than mathematical exactness. Though this does create a different point of view, it in no sense does away with the need so well presented by Wilson for knowledge of inherent mathematical relationships as a prerequisite to a statistical investigation. The mathematical tool is sharp and I may nick, or may already have nicked, myself with it. At any rate I do still enjoy the fun of whittling and of fondling such clean-cut chips as Wilson has let fall.

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ADLER, A.; AVELING, F.; BEKHTEREV, V.; BENTLEY, M.; BRETT, G. S.; BÜHLER, K.; CANNON, W. B.; CARR, H. A.; CLAPARÈDE, ED.; DUNLAP, K.; GAULT, R. H.; GRUEHN, D. W.; HOISINGTON, L. B.; HOWARD, D. T.; JAENSCH, E.; JANET, P.; JASTROW, J.; JÖRGENSEN, C.; KATZ, D.; KIESOW, F.; KRUEGER, F.; LANGFELD, H. S.; MCDOUGALL, W.; PIÉRON, H.; PILLSBURY, W. B.; PRINCE, M.; SEASHORE, C. E.; SPEARMAN, C. E.; STERN, W.; STRATTON, G. M.; TERRY, J. S.; WASHBURN, M. F.; WEISS, A. P.; WOODWORTH, R. S. Feelings and emotions: the Wittenberg symposium. (Martin L. Reymert, Ed.) Worcester, Mass.: Clark University Press, 1928. Pp. xvi+454. $6.00.

BEARD, C. A. (Ed.) Whither mankind. New York: Longmans, Green, 1928. Pp. vii+408. $3.00.

BOUSFIELD, W. R. The basis of memory. New York: Norton, 1928. Pp. 103. $1.00.

CARMAN, H. J., LANGSAM, W. C., & WOOD, B. D. American Council European history test. Yonkers: World Book, 1928. $1.50; spec. set, $.25.

CHADDOCK, R. E., & CROXTON, F. E. Exercises in statistical methods. Boston: Houghton Mifflin, 1928. Pp. vi+166.

COPELAND, E. B. Natural conduct. Stanford: Stanford Univ. Press, 1928. Pp. vii+262. $3.50.

DASHIELL, J. D. Fundamentals of objective psychology. Boston: Houghton Mifflin, 1928. Pp. xviii+588. $3.00.

ENGLISH, H. B. A student's dictionary of psychological terms. Yellow Springs, Ohio: Antioch Press, 1928. Cloth, $1.00; paper, $.65.

HOLLINGWORTH, L. S. The psychology of the adolescent. New York: Appleton, 1928. Pp. xiv+227. $2.50.

JANET, P. L'évolution de la mémoire et de la notion du temps: I. La durée. Paris: Chahine, 1928. Pp. 173. 18 fr.

———. L'évolution de la mémoire et de la notion du temps: II. La mémoire élémentaire. Paris: Chahine, 1928. Pp. 173-419. 26 fr.

———. L'évolution de la mémoire et de la notion du temps: III. L'organization du temps. Paris: Chahine, 1928. Pp. 419-625. 21 fr.

JOHNSON, H. M. Children in the nursery school. New York: Day, 1928. Pp. xxii+325. $3.00.

LAIGNEL-LAVASTINE, M. La méthode concentrique dans l'étude des psychonévrosés. Paris: Chahine, 1928. Pp. 285.

LEARY, D. B. Modern psychology: normal and abnormal. Philadelphia: Lippincott, 1928. Pp. xiv+441. $4.00.

LEIGH, R. D.; MCGOLDERICK, J. D.; ODEGARD, P. H.; & WOOD, B. D. American Council civics and government test. Yonkers: World Book, 1928. $1.50; spec. set, $.25.

LOW, B. Psycho-analysis and education. New York: Harcourt, Brace, 1928. Pp. 224. $2.25.

MACCURDY, J. T. Common principles in psychology and physiology. New York: Macmillan; Cambridge: Cambridge Univ. Press, 1928. Pp. xvii+284.

MARCU, V. Lenin: thirty years of Russia. New York: Macmillan, 1928. Pp. 412. $5.00.

MUKERJEE, R., & SEN-GUPTA, N. N. Introduction to social psychology. New York: Heath, 1928. Pp. xv+304. $3.00.

METFESSEL, M. Phonophotography in folk music. Chapel Hill: Univ. of North Carolina Press, 1928. Pp. x+181. $3.00.

MURPHY, G. An historical introduction to modern psychology. New York: Harcourt, Brace; London: Kegan Paul, 1928. Pp. xvii+470. $6.00.

NEEDHAM, J. Man a machine. New York: Norton, 1928. Pp. 103. $1.00.

PAVLOV, I. P. Lectures on conditioned reflexes. New York: International Publishers, 1928. Pp. 414. $6.50.

PIÉRON, H. Thought and the brain. (Trans. by Ogden, C. K.) New York: Harcourt, Brace; London: Kegan Paul, 1927. Pp. xvi+262. $4.00.

RAUDENBUSH, H. W., SICELOFF, L. P., & WOOD, B. D. American Council solid geometry test. Yonkers: World Book, 1928. $1.25; spec. set, $.20.

———. American Council trigonometry test. Yonkers: World Book, 1928. $1.25; spec. set, $.20.

SCHJELDERUP, H. K. Psychologie. Berlin: Gruyter, 1928. Pp. ix+330.

SEARLES, H. L. The study of religion in state universities. *Univ. Iowa Stud.: Stud. in Character,* 1928, **1**, No. 3. Pp. 91.

SLAGHT, W. E. Untruthfulness in children: its conditioning factors and its setting in child nature. *Univ. Iowa Stud.: Stud. in Character,* 1928, **1**, No. 4. Pp. 79.

STERN, W. Psychologie der frühen Kindheit. (Rev. ed.) Leipzig: Quelle & Meyer, 1928. Pp. xv+539. M. 12.80.

STINCHFIELD, S. M. The psychology of speech. Boston: Expression Co., 1928. Pp. xiv+331. $3.75.

TAYLOR, H., BARROWS, T. N., & WOOD, B. D. American Council economics test. Yonkers: World Book, 1928. $1.30; spec. set, $.25.

ZIMMERN, A. Learning and leadership. London: Oxford Univ. Press, 1928. Pp. 110. 5*s*.

# PSYCHOLOGY IN THE U. S. S. R.*

*From the Gosudarstvennii Institut Eksperimentalnoi Psikhologii, Moskva*

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In order to give an outline of the present status of psychology in the U. S. S. R. it is necessary, first of all, to speak of the position occupied by the State Institute for Experimental Psychology (S. I. E. P.) in Moscow. We are justified in thus approaching this question, first, by the fact that the S. I. E. P. is the only special psychological institution, and, numerically, the most important scientific center of psychology in our Union; and, second, because the methodological principles which the Institute laid down as the basis of its work are characteristic of life in general in the Soviet Union, and of all the most important leading scientific tendencies.

The position of the S. I. E. P., in its turn, may be more definitely described as the struggle of two (different) fronts.

At the moment when the Institute sprang into being in 1923, it met with one of these fronts, or, rather, the origin of the Institute was one of the phases of a struggle which had been commenced earlier by Professor K. N. Kornilov. At that time the struggle was mainly directed against the tendency in psychology which prevailed in Moscow University. This tendency Professor Kornilov characterized "the metaphysical conception of empiric psychology." This was a form of psychology having as its aim the study of "mental states and phenomena" independent of physiological processes, by means of introspection. If experiments were applied, it was merely as starting-points of introspective speculations. It is quite evident that with such aims and methods this form of psychology had lived its day and had long since been substituted in all the most advanced countries by new and powerful tendencies. Another very important circumstance should be pointed out. This psychology had as its basis metaphysical, philosophical conceptions, a thoroughly idealistic philosophy opposed to any materialism whatever. Now, again, it was clear that such ideas, under the conditions of the present day, were anachronisms and differed widely from the principles which

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permeated life in our Union, and which gained a dominant place among all other disciplines.

The outstanding problem was to work out a form of psychology on principles of dialectical materialism, in opposition to the idealistic philosophy and to subjectivism in psychology.

And this task the S. I. E. P. set as its aim. Professor Kornilov formulated the postulates as follows. To begin with, *materialism* does not regard the psychic as something transcendental, nor as primary and independent of material processes, nor as a series of psychic phenomena running parallel with and mysteriously coordinated to a series of physical phenomena,[1] but as one of the properties of complex material systems, as a quality peculiar to matter at the highest stages of its organization. The materialist does not acknowledge non-material phenomena; he finds only one single series of real processes; but the singleness of this series does not exclude the possibility of approaching it in different, non-identical ways; and one of them—the introspective standpoint ("the introspective expression of physiological processes") is what we term "psychics." Then there is the "determinism" of human behavior, and Professor Kornilov emphasizes, above all, the "determinism by social factors." Such are the fundamental divergencies with the first group of opponents—the idealists and indeterminists. This was the reason for the widely different views as to the rôle of scientific experiments in psychology. We said that in subjective psychology it served only as a motive for speculations. The S. I. E. P. considers the facts obtained by the objective method as being fundamental, and facts of introspection, not confirmed by objective investigation are not acknowledged as reliable. Objective facts have the priority, though introspection is not completely excluded from psychology. The third "and, perhaps, the most important" postulate, is the *dialecticalness* of psychology. And here we pass over to the second front, where a fierce struggle is going on against tendencies entirely rejecting psychics, identifying it with physiological processes or mechanical movements, ignoring the qualitative differences between biological phenomena and what we call psychics. These same tendencies usually set at nought the importance of social factors conditioning human behavior. The actual struggle, in this direction, was chiefly against reflexologists, or,

[1]E. B. Holt is quite right when he terms psychophysical parallelism the greatest bluff of the twentieth century.

more precisely, it was against the exaggeration of the part played by reflexology in interpreting human behavior. Though we by no means deny the immense importance of studying reflexes, their co-ordinations and connections, we cannot agree that the study of human behavior ends at this. Phenomena of a more complex nature are following their own peculiar laws, and the most extensive knowledge of laws of simpler phenomena will never succeed in removing these laws, for the more complex phenomena *as such* possess peculiar features which have to be studied in their peculiarity; and these disappear if we begin to reduce the more complex phenomena to their components or to dissect them into elements. And such interrelations can be accepted and understood only by one who is equipped with dialectics. Without this instrument the investigator is condemned to schematizing and mechanizing complex phenomena, to consciously or unconsciously simplifying them, and to closing his eyes to their specific properties.

The position of the contending parties on the second front is quite different from those on the first. We had there a wide gap of entirely opposite points of views and radical dissensions. Here the divergencies are mostly on the question of mutual demarcation, of limits in the application of one or the other method. As long as reflexology remains within its legitimate boundaries, the psychologist does not fight against it, but, on the contrary, he includes all its inferences in his inventory. The contention commences from the moment when the reflexologist, forgetting about the qualitative peculiarity of the field studied by psychology, attempts to sweep it completely away.

It would seem that on the first front where the sides are in direct opposition the contest should be more intense than on the second, where the dispute, so to say, is with hyperbolizing collaborators—whereas in reality it is far from being thus.

The immense authority and widespread face which our academician Pavlov very justly deserves in our scientific world forces the audience to be deeply interested in all the truly excellent work which we owe to the Pavlov school. But this same excellent work, involuntarily, produces an especially trustful and not very critical attitude towards everything, in general, coming from physiological spheres; such is the attitude towards certain exaggerations for which some of the academician Pavlov's co-workers are to blame. Moreover, by such an

unsound credulity are profiting to a considerable measure the works of other physiologists as well, for instance, the impetuous generalizations of Bekhterev and others. All this, in the eyes of the public, shines with the reflected light of Pavlov's sun.

The consequence is that hyperenthusiastic physiologists attempt to overstep the legitimate boundaries in the field set apart for them. Workers in several other sciences, struck with the apparent success of the physiologists, believe they can obtain speedy results by putting their special concepts into physiological language, groundlessly and superficially physiologizing and biologizing their own disciplines. Of course, this cannot possibly be done without ignoring the specific peculiarities of complex phenomena mentioned above; nevertheless such attempts have become very frequent.

It is evident that here we have a fight for the very existence of psychology as a separate discipline. The dialectical materialist is constrained to prove to the non-dialecticians and anti-dialecticians that human behavior in all its specific complexity, conditioned as it is by social factors, cannot be mastered by physiology only; he has to prove that all the qualitative peculiarity of human behavior would be lost through an attempt to resolve it into reflexes; that physiology and reflexology both have to deal with the human being, as a representative of the *homo sapiens* species, with "man in general," whereas psychology deals with men having certain habits and traditions, the ideology of their class, profession, level, etc; and, last of all, that it is for this very reason that his object has to be studied by methods, perhaps similar, but still somewhat specific and peculiar.

We say that the psychologist considers man as a social being; he studies his reactions as the reactions of a social unit; he is interested in these reactions and their mechanisms as well; but he studies also the retroactive effect of the contents of the reaction on the mechanisms of behavior, for he considers that these are also conditioned by social factors. The psychologist will never agree with the reflexologist, who completely rejects introspection. The psychologist cannot do without introspection altogether, for the reason that he comes upon many of his most important problems by means of introspection. Of course the materialistic psychologist will attend to these problems by objective methods, which alone can give reliable results, but this does not diminish the rôle which introspection has played. The mind of man will be studied in its objective manifesta-

tions but will not be identified with those external manifestations and, of course, it will not be ignored altogether, as it has to be ignored by physiology. Thus, it is clear that the struggle for psychology, under the existing conditions, is at the same time a struggle for a dialectical conception of the universe.

From the standpoint of the S. I. E. P., psychology can be only dialectical, otherwise a lapse into idealism or else into the other extreme—the complete mechanization of human behavior—is inevitable.

An adherent of dialectical materialism cannot throw overboard the whole of psychology; however negative his attitude is towards subjective and speculative varieties of psychology and towards their philosophic background, he will, nevertheless, try to make use of all the facts obtained by them. If we entirely get rid of psychology and put reflexology in its place, it would mean either, as we have already said, the rejection of dialectics or an arbitrary introduction of new terminology; we should then begin to call reflexology what we used to design as psychology, i. e., the science of human behavior conditioned by social environment.

As the fundamentals of dialectical materialism were worked out by the classic authors of revolutionary Marxism, of course, Professor Kornilov, from the outset, had to lean on Marx and Engels, though they have very little to say about psychology.

The connection between Marxism and psychology was first suggested by Professor Kornilov (at the first Psychological Congress in Moscow). His views were afterwards given in a number of articles which appeared in the journal *Pod znamenem Marksisma,*[2] and in the memoirs of the S. I. E. P., entitled "Psychology and Marxism" and "Problems of Modern Psychology," also in a separate book, *Modern Psychology and Marxism.* The book by Professor Kornilov, *The Science of Human Reactions,* (reactiology), in which he has brought together the experimental work done by him before the year 1921, has been issued in 1927 in a third edition, supplied with the results of later experiments. Professor Kornilov's harmonious system of experimental investigations is devoted to the study of "the reactions of man as a whole, of man as a concrete living organism"; the object of the study of these reactions was not only to examine their qualitative aspect but to give a quantitative

[2] *Under the Banner of Marxism.*

characteristic as well. This quantitative characteristic includes all the three moments existing in each reaction—time, intensity, and form of the motion. In the third volume of *The Problems of Modern Psychology* is published an article by Professor Kornilov on the "Explosive Reactions." Besides these, there have been published *A Manual of Psychology* and *An Outline of Pre-School Child Behavior* by Professor Kornilov. The above-mentioned memoirs of the S. I. E. P. contained also works by the following, former and present, collaborators of the Institute: M. A. Reissner, A. R. Luria, A. N. Zalmanson, V. A. Artemov, B. D. Friedman, V. M. Borovski, L. S. Vygotski, P. P. Blonski, A. H. Leontev, I. N. Spilrein, S. G. Gellerstein, N. A. Rybnikov, N. F. Dobrynin, N. E. Akimov, A. S. Chernishov.

The work of building a psychology on the principles of dialectical materialism can by no means be considered as accomplished. According to Professor Kornilov, "Many a generation of scientists will still be engaged with the solution of this problem." The most difficult task of the present moment is to fuse organically the theoretic principles with experimental practice. Taking into consideration this end the S. I. E. P. centers its work around the problems of collective and class psychology. The subject is never viewed as "man in general" but as the representative of a certain social class.

Four sections are at present working at the S. I. E. P. The most numerous and the leading section—*the section of general psychology*—is headed by Professor Kornilov, director of the Institute. This section studies the three moments of reactions, already mentioned above, in connection with the study of a collective, or class. Next comes *the section of child psychology*, at the head of which is N. A. Rybnikov; this section studies the habits of children, and mainly the child's speech. *The section of zoöpsychology*—with V. M. Borovski as the chief—studies a series of questions dealing with the problem of habit formation in animals. *The section of psychotechnics*—administered by I. N. Spilrein—works on questions of methodology of psychotechnical investigations, and also on questions of professiography, fatigue, and training. Besides the works published in the Soviet and foreign journals, the collaborators of the S. I. E. P. have issued, during recent years, the following works of greater importance: *Child Language* by Rybnikov, *Pedagogic Psychology* by Vygotski, *Introduction to Comparative Psychology*

by Borovski, *Natural Experiment* by Artemov, *A Practical Course in Experimental Psychology,* and *Readings in Psychology* by Artemov, Bernstein, Vygotski, Dobrynin, Luria, *The Statistical Method in Psychology and Pedology* by Belev-Bashkirov,[3] *Fluctuation of Attention* by Dobrynin, and others.

On questions of general psychology the following books have come out: *An Outline of Scientific Psychology* and *Psychological Outlines* by Blonski, *Outlines of Collective Psychology* by Voytolovski. A periodical, *The Journal of Psychology, Psychotechnics, and Pedology,* has been founded by the S. I. E. P., and the workers of all the Union are invited to collaborate.

Passing over to other Institutes not especially psychological, it is necessary to mention the Leningrad Institute of Scientific Pedagogics, which has issued the following books: *Psychology, Reflexology, and Physiology* by Basov; *After Empiricism* and *The Principal Tendencies in Foreign Psychology* by I. V. Evergetov; and the Ukraïne Psychoneurologic Institute in Kharkov. The latter has a Department of Experimental Psychophysiology where Z. I. Chuchmarev is carrying on intensive work in experimental psychology. It is at Kharkov also that very interesting works have appeared on the question of methodology of psychotechnics (Syrkin, Mandryka).

During the last few years the psychologists of the idealist camp have issued several works by Nechaev, and several pamphlets by G. I. Chelpanov, in which the latter, criticizing the fellows of the S. I. E. P., tries to prove that only empiric psychology agrees with the principles of Marxism.

It is not the object of this note to give an outline of the work in reflexology. It is of interest to mention Bekhterev's attempt to prove that reflexology was always a dialectical discipline.

We have to emphasize a very characteristic tendency of Soviet science in general and of psychology as well, which is the intrinsic interest in problems of applied character. Among this group we should mention the names of workers in the sphere of professiography: Spilrein (Moscow), Dunaevski (Kharkov), Yurovskaya (Kazan), Gaivorovski (Minsk); in professional selection and other problems of industrial psychology: Spilrein, Gellerstein, Rudik, Kogan, Spigel, Nayfakh, Kekchaev, Tolchinski, Karpov, Smirnov,

[3]In recent years the term "pedology" has been applied to the science of the child, in general, or the science of child behavior, in particular.

and others. Furthermore, we have the important and continually growing work of controlling the experimental schools for adults; this work is carried on by a number of collaborators of the S. I. E. P. Then there is the work dealing with medical questions: Chuchmarev, Sapir, and others; questions of pedagogics: Blondski, Rybonikov, Basov, Zalkind, Moloyavi, Boltunov, Evergetov, Griboedov, Kashenko, Veselovskaya, Fortunatov, Zaluzhni, and others; questions having to do with the textile industry: Kornilov and his collaborators; the cinema: Rudik.

In conclusion we will try to formulate briefly the attitude existing here towards the main tendencies of foreign psychology. We may say, with confidence, that subjective empiric psychology has no success and its partisans are but very few.

Some of our junior fellows are showing considerable interest lately in the German Gestalt psychology. There is no doubt that the dialectical Marxistic psychology has many points in common with it. Certain principles in it rather conform to fundamental dialectical postulates. On the other hand, subjectivism and idealism, which are very prominent in Gestalt psychology, cannot, of course, be approved of. It is difficult to formulate in few words our attitude towards American behaviorism. The majority of psychologists of our Union owe a great deal to behaviorists; we loaned and learned a lot from them. But we shall never agree either with a behaviorism trying to be purely empirical (for "no philosophy means bad philosophy") or with a behaviorism tending to eclectics—not until behaviorism is rebuilt on the philosophical basis of dialectical materialism.

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## LA PSYCHOLOGIE DANS U.S.S.R.
(Résumé)

La plus grande institution centrale consacrée surtout à l'etude de la psychologie est l'Institut de l'Etat pour la Psychologie Expérimentale, à Moscou. Cette institution a choisi comme seul travail le développement d'un système de psychologie basé sur les principes du matérialisme dialectique. Les grands traits caractéristiques de la tendance de la I.E.P.E. sont: le matérialisme dialectique; le déterminisme; une grande tendance à étudier la psychologie collective et la psychologie des classes; l'étude du sujet non comme représentatif de l'espèce *homo sapiens,* mais plutôt comme représentatif d'une certaine classe sociale; l'introspection seulement comme moyen de cette recherche objective qui peut seule donner des faits scientifiques. Le devoir le plus important de l'institut est la fusion des principes théoriques et de la technique expérimentale. Le directeur de l'institut et le chef de

département le plus important est le Professeur K. N. Kornilov. Ce département étudie les réactions de l'homme comme un organisme concret et vivant avec sa complexité à l'égard des traits caractéristiques qualitatifs et quantitatifs, tels que le temps, l'intensité, l'espèce de motion, etc. Le département de la psychologie des enfants (Directeur, M. A. Ribnikov) examine la parole et les habitudes de la parole des enfants. Le département de la psychologie des animaux (V. M. Borovski) examine le procédé de l'apprentissage chez les animaux. Le département de la psychotechnique (I. N. Spilrein) examine les problèmes méthodologiques, la pratique professiographique, et la fatigue. Parmi les institutions non surtout consacrées à la psychologie les suivantes font quelques recherches psychologiques: l'Institut de Pédagogie Scientifique à Léningrad (M. Y. Basov, I. N. Evergetov) et l'Institut Psychonéurologique de l'Ukraine à Kharkov (Z. I. Chuchmarev). A Kharkov aussi, Sirkin et Mandrika étudient les problèmes méthodologiques de la psychotechnique; la professiographie—Dunaevski (Kharkov), Yurovskiya (Kazan), Gaivorovski (Minsk). Il se manifeste un grand intérêt aux problèmes psychologiques qui ont une application pratique. On indique brièvement le rapport à la psychologie des formes et au behaviorisme.

BOROVSKI

## PSYCHOLOGIE IN DER U.S.S.R.

(Referat)

Das grösste zentrale und speziell dem Studium der Psychologie gewidmete Institut ist das Staatsinstitut für Experimentelle Psychologie in Moskau. Das Institut sieht seine Lebensaufgabe in der Ausarbeitung eines Systems der Psychologie auf Grund der Prinzipien des dialektischen Materialismus. Bezeichnend für die Richtung des S.I.E.P. sind—dialektischer Materialismus; Determinismus; Einstellung auf das Studium der Kollektiv—und Klassen-Psychologie; Studium der Versuchsperson, nicht als Vertreter der Species homo sapiens, sondern als Vertreter einer bestimmten Gesellschaftsklasse; Introspection nur als Hilfsmittel einer objektiven Forschung, die allein wissenschaftliche Tatsachen liefern kann. Eine erste Pflicht für das Institut ist eine Verschmelzung der theoretischen Prinzipien mit der experimentellen Technik. Direktor des S.I.E.P. und Leiter der dominierenden Abteilung "für allgemeine Psychologie" ist Prof. K. N. Kornilow. Die Abteilung erforscht Reaktionen des Menschen, als eines konkreten lebendigen Organismus in seiner komplexheit, hinsichtlich ihrer qualitativen und quantitativen (Zeit, Intensität, Form der Bewegung) Merkmale. Die Abteilung für Psychologie des Kindes (Leiter—N. A. Rybnikow) untersucht die Sprache und Sprachgewohnheiten des Kindes. Abteilung für Tierpsychologie (W. M. Borowski)—den Lernprozess bei Tieren. Abteilung für Psychotechnik (J. N. Spielrein)—methodologische Probleme, Professiographic, Einübung und Ermüdung.—Von den nicht speziell psychologischen Instituten beschäftigen sich mit Psychologie: das Institut für wissenschaftliche Pädagogik in Leningrad (M. J. Bassow, J. W. Ewergettow) und das Psychoneurologische Institut der Ukraïne in Charkow (S. J. Tschutschmarew). Ebenfalls in Charkow studieren methodologische Probleme der Psychotechnik, Syrkin und Mandryka; Professiographie—Dunajevski (Charkow). Jurovskaja (Kasan), Gaiworowski (Minsk).—Hervorgehoben wird ein dringendes Interesse an solchen psychologischen Problemen, die eine praktische Anwendung finden. Das Verhältnis zur Gestalpsychologis und zum behaviorismus wird kurz formuliert.

BOROWSKI

## ПСИХОЛОГИЯ В. С. С. С. Р.

(Реферат).

Центральным научным учреждением по психологии в СССР является Государственный Институт Экспериментальной Психологии в Москве. Основная позиция Г. И. Э. П.—разработка психологии, основанной на принципах диалектического материализма. Диалектический материализм, детерминизм, изучение коллективной и классовой психологии, человек — всегда как представитель своего класса, а не как „человек вообще", самонаблюдение, как вспомогательное средство, а об'ективный метод, как единственный, дающий надежные результаты — вот характерные черты направления Г. И. Э. П. Задачей момента является вработка теоретических положений в экспериментальную технику. Во главе Г. И. Э. П. и его руководящей секции общей психологии стоит проф. К. Н. Корнилов. Секция изучает реакции живого, конкретного и целостного человека с их качественной и количественной сторон. Количественная характеристика охватывает быстроту, интенсивность и форму движений. Секция детской психологии (Н. А. Рыбников) изучает язык и речевые навыки ребенка. Секция зоопсихологии (В. М. Боровский)—проблему обучаемости у животных. Секция психотехники (И. Н. Шпильрейн)—методологию психотехники, вопросы профессиографии, упражнения и утомляемости. Из других институтов психология изучается в Ленинградском Институте Научной Педагогики (М. Я. Басов, И. В. Эвергетов) и в Украинском Психоневрологическом Институте в Харькове (З. И. Чумарев). По методологии психотехники в Харькове работает Сыркин, Мандрыка. По профессиографии—Дунаевский (Харьков), Юровская (Казань), Гайворовский (Минск). Подчеркивается сугубый интерес к тем проблемам психологии, которые имеют прикладное значение. Кратко формулируется отношение к Gestaltpsychologie и бихэвиризму.

**Боровский** (Borovski)

# GEOTROPIC ORIENTATION OF CHICKS*

*From the Laboratory of General Physiology, Harvard University*

HUDSON HOAGLAND†

## I

Crozier and Pincus (3) showed that young rats while creeping on an inclined plane orient upward in such a way that the average extent of orientation, as measured by the angle ($\theta$) which the path of the rat makes with the lower edge of the rectangular plane, is very nearly proportional to the logarithm of the sine of the angle of inclination of the plane ($\alpha$). This latter factor is equivalent to the logarithm of the gravitational component in the plane.

From an analysis of the geometry of the rat and of the forces acting during its progression, they were able to account for the observed paths in terms of equations based on a muscle-tension theory of the limitation of orientation. This theory was further tested by attaching weights to the creeping animals, thereby altering the differential muscle tensions and the resulting paths of creeping in a way predicted by the equations from the proprioceptive theory of orientation (Crozier and Pincus, 4; Pincus, 15). The analysis has since been extended and confirmed (cf. Crozier, 1).

Wolf (16) examined the behavior of *Agriolimax* on a plane inclined at various angles to the horizontal and likewise found a linear relation between the angle of orientation ($\theta$) and the logarithm of the gravitational component (sine $\alpha$) in the creeping plane. Since the geometry of slugs is so different from that of rats, it might at first seem difficult to account for the same law in both cases. A careful consideration of the problem, however, shows that a proprioceptive theory of orientation is altogether adequate in each case. The formulation as concerns the slugs may be based on the hypothesis that the geotropic orientation is limited by the threshold difference between the pull of the body mass on the mutually inclined longitudinal muscles at the anterior end of the slug.

Crozier and Oxnard (2), working with young mice on an inclined

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plane, were able to show that these animals also orient according to the equation previously found to apply to rats. Crozier and Stier (8, 9) have extended this type of analysis to the consideration of various other forms (cf. Crozier, 1); Kropp and Crozier (13) demonstrated the applicability of the general proprioceptive theory of geotropic orientation in the case of the fiddler crab *Uca,* as did also Kropp (12) in the case of the beetle *Pyrophorus.*

Hunter (10) examined the behavior of young rats before the eyes had opened and also of blinded adult rats on an inclined plane. He found, however, that under the conditions of his test most of the animals climb the plane in the line of steepest slope and that the number so doing increases with the slope of the plane, the others taking various directions. Hunter found much less regularity in behavior than that reported by Crozier and Pincus. The means of the angular deviations from the horizontal were found to increase with the logarithm of the sine of the angle of slope, but actually he found that only a very few animals show the mean angle. From these results he concluded that the relations previously stated by Crozier and Pincus is a measure of the frequency of the rats to respond to geotropic stimulation rather than the expression of a law of reception.

Piéron (14), as a result of work with slugs, likewise disagrees with the proprioceptive hypothesis of orientation. The conditions of his experiments are apparently unsuitable, however, to serve as a basis of comparison with those of Wolf. By placing the slugs on a plane immersed in water and thereby annulling most of the effect of gravity, he states that the animals consistently take the line of greatest slope above a threshold value of $\alpha$. His data disagree with those obtained by Wolf, and he concludes that orientation is most probably determined by mechanisms of the statocyst.

The rats used by Crozier and Pincus in there original experiments were as nearly similar as possible. Littermates from lines long inbred were used under externally uniform conditions. An important feature consists in the exclusion of the effects of other kinds of excitation, notably that due to light (Crozier and Pincus, 5; Keeler, 11). As a corollary to their findings on rats of one type (*Rattus norvegicus*), the possibility became apparent that certain genetic or specific differences might find expression by means of the constants in the equations for geotropic behavior. Crozier and

Pincus (6) examined the orientation of *Rattus rattus* (roof rat), and found that it obeys the equations that apply to *Rattus norvegicus* but that the former is geotropically more sensitive and that the numerical values of the constants in the equations for the two forms differ significantly.

A still more recent paper by these authors (7) shows clearly that "constants in equations of curves describing the geotropic orientation of young rats are repeatedly obtainable from litters of successive generations of the same inbred strains. Different inbred strains have been obtained for which the respective constants are quite unlike." These results indicate the importance of employing biologically uniform material in experiments of the kind in question.

The work of Crozier, Pincus, Wolf, Oxnard, Stier, and Kropp presents a strong case for a general proprioceptive theory of the limitation of geotropic orientation, owing to the very diverse variety of forms used, quadrupeds, arthropods, and slugs. To examine further the general case, it seemed desirable to select an animal differing widely in its mode of progression and support from those previously chosen. Bipeds seemed to be interesting in this connection, and very young chicks were accordingly selected as typical and convenient for the purpose.

## II

A group of six three-day-old chicks was tested on an inclined plane. Two of these were Barred Plymouth Rocks, one was a White Plymouth Rock, and three were Rhode Island Reds. A hinged plane 35 x 35 inches ruled off in four-inch squares was used. Its slope was adjusted by a cord and pulley system and the angle $a$ was measured by a large protractor. The surface of the wooden plane was covered with black cloth, over which was fastened a wire screen to prevent slipping. The experiments were conducted in the dark, and spots of luminous paint were placed on the back of each chick and on the top of the head. One-inch luminous strip markers were also pasted around the four sides of the plane, to locate the four-inch coordinates. Records of the runs were kept by tracing the paths of the chicks on ruled coordinate paper duplicating the coordinates of the plane. The paper was placed on a box with a glass lid which contained a very dim red light which transfused through the paper without illuminating surrounding objects.

It was found that the chicks orient quite definitely when walking

on an inclined plane in the dark. With some individuals great difficulty was encountered from the fact that they may remain motionless on the plane for half an hour at a stretch "cheeping" vigorously. These birds were either discarded or were used for only a few determinations. The three Rhode Island Reds were the most active on the plane and gave the most complete groups of data. For the most part the chicks were negatively geotropic, but one individual was found which was consistently positively geotropic. In the cases of the negatively geotropic birds, 5 to 10% of the trails were down the plane, and these trails made approximately the same angle $\theta$ with the horizontal as did the upward trails for the same slope of the plane. (Other similar cases have been observed; cf. Wolf, 16.)

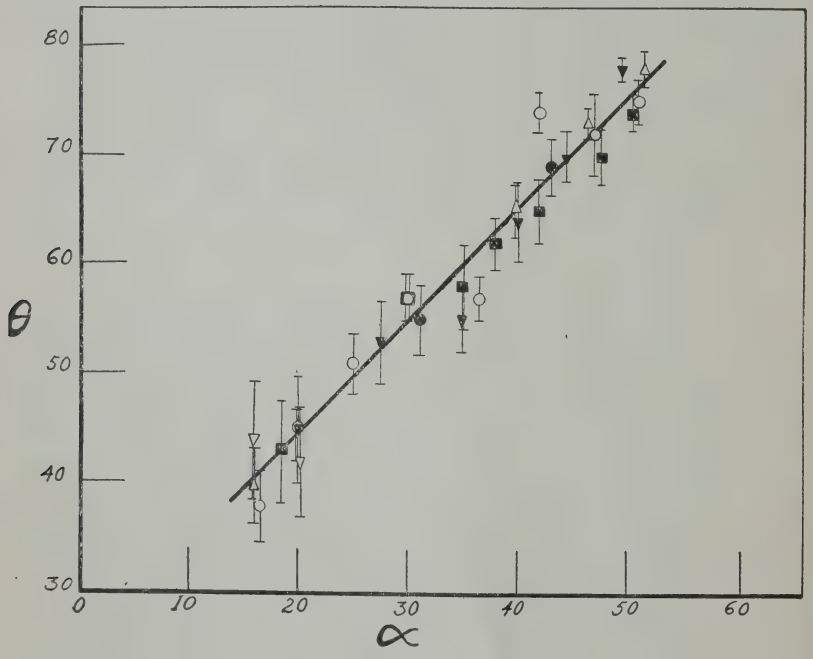


FIGURE 1

PLOT OF $\alpha$ THE ANGLE OF INCLINATION OF PLANE, AGAINST $\theta$, THE ANGLE MADE BY THE PATH OF THE CHICK WITH THE HORIZONTAL LOWER EDGE OF THE PLANE

The marked circles, the squares, and the shaded inverted triangles represent data obtained from three Rhode Island Reds. The points are means of an average of fourteen trials.

Figure 1 gives the relation between the angle of upward orientation ($\theta$) and the slope of the plane ($\alpha$). Only upward trails are recorded in this figure; the few downward trails did not furnish enough data for analysis. It may be noted that, since the object of the tests is to ascertain the laws limiting the path of upward creeping, this sort of selection is entirely legitimate. Each point is the mean of an average of 14 paths. The probable errors of $\theta$ are recorded as vertical lines through the points. It may be seen that, in general, the probable errors decrease as $\alpha$ increases (cf. Crozier and Pincus, 3). This relation is only approximate, however, since the number of trials for each point is not constant.

## III

If the chick were oriented in its upward path solely by mechanisms of the inner ear, one might expect, as Piéron has suggested for the case of slugs, that the bird would climb the path of greatest slope, i.e., would go straight up the plane when $\alpha$ exceeds a threshold value. This is certainly not the case here, and the quantitative relation between $\alpha$ and $\theta$ immediately suggests some form of proprioceptive theory.

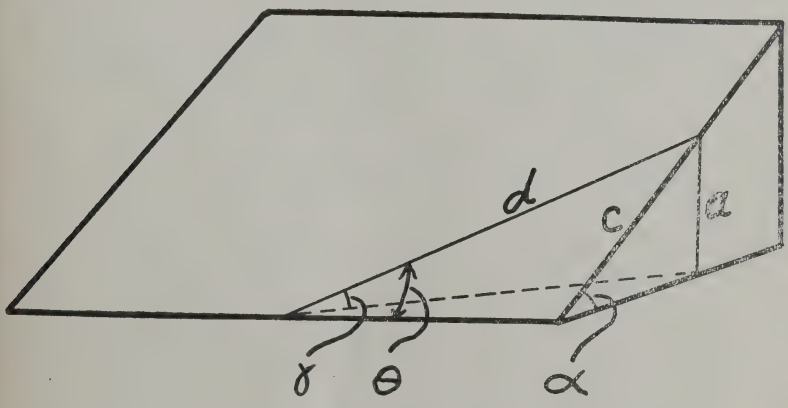


FIGURE 2
THREE-DIMENSIONAL REPRESENTATION OF THE EXPERIMENTAL PLANE SHOWING RELATIONS OF THE ANGLES $\alpha$, $\theta$, AND $\gamma$

Figure 2 is a three-dimensional representation of the experimental plane, illustrating the angles $\alpha$, $\theta$, and $\gamma$, this last being the angle of the path of progression with the horizontal.

The sin of $\gamma$ is equal to $\frac{a}{b}$

$$\text{and } \sin \theta = \frac{c}{d}$$

$$\sin \alpha = \frac{a}{c}$$

$$\text{Therefore } c = \frac{a}{\sin \alpha} = d \sin \theta$$

$$\sin \theta = \frac{a}{d} \quad \frac{1}{\sin \alpha}$$

$$\sin \theta = \frac{\sin \gamma}{\sin \alpha}$$

$$\sin \gamma = \sin \alpha \sin \theta$$

It was noticed that when the bird is placed on the plane it stands vertically, regardless of the magnitude of $\alpha$. This maintenance of an upright posture is of an all-or-none character and is most probably influenced by the action of gravity on ear mechanisms. The legs are long and near together, and it may be assumed that a line passing through them vertically would pass through the center of gravity of the bird when $\alpha = 0$. At the beginning of each step as $\alpha$ increases, the chick must flex the leg which is higher up the plane in order to maintain its vertical body position, the center of gravity remaining over the lower leg. The center of gravity cannot, of course, pass beyond a position directly over the "down" leg or the bird would tend to topple over backwards (cf. Kropp and Crozier, 13).

Figure 3 is a schematic representation of the chick on the plane. As $\gamma$ increases from 0° to 90° sin $\gamma$ also increases from 0 to 1, since the length of the extended lower leg is constant; this means that the leg on the up side must flex, thereby decreasing $x$, the distance between the center of gravity and the plane. The length of the stride must also increase with an increase in $\gamma$. Therefore, as sin $\gamma$ increases, the stretching of various groups of muscles will also increase. But sin $\gamma = \sin \alpha \sin \theta$, which is equivalent to saying that the stretching increases as the product of the sine of the angle of the plane with the horizontal, and the sine of the angle of orientation in the plane. This is consistent with the empirical finding that $\alpha$ increases with $\theta$.

In Figure 3, since we are merely concerned with the distance between the plane and the bird, only one leg joint, the ankle, is represented. Actually there are three joints, the ankle, the knee, and the hip, which presumably may play a part in the orientation.

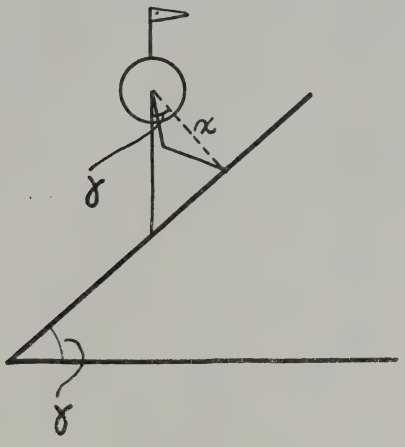


FIGURE 3
SCHEMATIC REPRESENTATION OF CHICK ON INCLINED PLANE
The distance $x$, between the bird and the plane is directly dependent on the degree of flexion of the "up" leg

## IV

To analyze better the mode of progression, a model of a chick was made, and the relations of the various joints were determined as a function of the angles $\alpha$ and $\theta$. The model is constructed of brass and consists of two "legs" jointed to correspond to the bird's ankles and bolted onto a crosspiece to represent the body of the chick. The juncture between the legs and body furnishes a joint combining the function of both hip and knee. A spirit level is screwed to the "body" along the line connecting the two upper joints, to enable one to adjust the model so that the line connecting the "hips" remains horizontal, corresponding to the bird's upright position on the plane. In addition, a plumb-bob swinging from the center of the "body" makes it possible to keep the center of gravity of the model over the line connecting the two "feet." This line is determined by an elastic band stretched between the feet.

In many of the geotropic studies by Crozier and his collaborators previously cited, it has been assumed that the animal turns until a certain differential threshold of tension is attained in the muscles of the two sides of the body, in this way limiting the angle $\theta$ in either the right- or left-hand quadrants. To test this hypothesis in the present case, the following experiment was performed with the model. Considering the muscle tensions to be proportional to the flexion of the joints, it should follow that if there is a constant threshold intensity for certain groups of leg muscles there should also be a constant angle of flexion of the leg joints during corresponding parts of the step for the experimentally determined values of $\alpha$ and $\theta$. Accordingly, a series of values of $\alpha$ were arbitrarily chosen from the data of Figure 1, and their corresponding $\theta$ values were noted. These angles are recorded in Table 1.

The model was then placed on a plane inclined at the chosen values of $\alpha$ in such a way that its "path of progression" would make the corresponding angle $\theta$ in the plane. The leg on the down side was kept at constant length by tightening the bolts of both joints, and the angle at the "ankle" of the upper leg was determined for corresponding values of $\alpha$ and $\theta$, allowing the leg freedom of movement during the adjustment at both of its joints. In addition, the body was kept horizontal, and the center of gravity was maintained above the line connecting the feet, this line being adjusted so that it was parallel to the line joining the "hips." Under these conditions the angle of the "ankle" of the upper leg was determined and is recorded in the third column of Table 1.

It may be seen that the "ankle" angle of the upper leg remains substantially constant over a range $\alpha = 16$ to $\alpha = 50$ degrees, which is essentially the range within which the chicks orient. Above and below this range the angle increases rapidly. These data, therefore, indicate a threshold of proprioceptive stimulation functioning to determine the angle of orientation in the biped used. This notion is not incompatible, as it might at first appear, with that suggested in the foregoing section. The argument developed there assumes a flexion of the upper leg, and this actually takes place at the upper joint of the model. It is only the so-called "ankle" joint that remains constant. To duplicate the conditions of Figure 3, the "thigh" joint of the model might have been fixed and the "ankle"

joint movable. For purposes of measurement it was, however, more convenient to use the experimental procedure just outlined.

TABLE 1

BEND AT "ANKLE" OF "UPPER" LEG OF CHICK MODEL FOR VALUES OF $\alpha$ AND $\theta$ CHOSEN FROM THE EXPERIMENTAL DATA OF FIGURE 1

The thigh joint of the "upper" leg is free to move while that of the "lower" leg is fixed. The "ankle" joint of the "lower" leg is also fixed arbitrarily at 165°.

| $\alpha$ degrees | $\theta$ degrees | Bend at ankle of upper leg degrees |
|---|---|---|
| 10 | 34 | 145 |
| 14 | 38.5 | 138 |
| 16 | 40.5 | 137 |
| 20 | 44.5 | 134 |
| 22 | 46.5 | 133 |
| 24 | 48.5 | 131 |
| 27 | 51.5 | 133 |
| 28 | 52.5 | 131 |
| 30 | 54.5 | 131 |
| 33 | 57.5 | 131 |
| 35 | 60 | 131 |
| 37 | 62 | 131 |
| 40 | 65 | 132 |
| 44 | 69 | 134 |
| 45 | 70 | 135 |
| 48 | 73 | 137 |
| 50 | 75 | 138 |
| 52 | 77 | 141 |

While it seems probable that the geotropic orientation of the chick depends on proprioceptive stimulation, the probable part played by ear mechanisms should not be lost sight of since the vertical position of the chick is necessary in order to bring about the bodily configuration resulting in the kind of proprioceptive control here suggested.

## SUMMARY

The orientation of chicks on an inclined plane shows a linear relation between the angular extent of upward orientation on the plane and the slope of the plane. This relation can be accounted for in the terms that the posture of the chick while walking on the plane results in the stretching of certain leg muscles, and consequently excites proprioceptive impulses.

The importance of the ear is emphasized as maintaining the posture necessary to the operation of a threshold difference in proprioceptive excitation in the muscles of the two legs.

## References


1. Crozier, W. J. Tropisms. *J. Gen. Psychol.,* 1928, **1**, 213-238.
2. Crozier, W. J., & Oxnard, T. T. Geotropic orientation of young mice. *J. Gen. Physiol.,* 1927-28, **11**, 141-146.
3. Crozier, W. J., & Pincus, G. The geotropic conduct of young rats. *J. Gen. Physiol.,* 1926-27, **10**, 257-270.
4. ———. Tropisms of mammals. *Proc. Nat. Acad. Sci.,* 1926, **12**, 612-616.
5. ———. Phototropisms in young rats. *J. Gen. Physiol.,* 1926-27, **10**, 407-418.
6. ———. Geotropic orientation of young rats. *J. Gen. Physiol.,* 1926-27, **10**, 519-524.
7. ———. On the geotropic orientation of young mamanls. *J. Gen. Physiol.,* 1927-28, **11**, 789-802.
8. Crozier, W. J., & Stier, T. J. B. Geotropic orientation in arthropods: I. *Malacosoma* larvae. *J. Gen. Physiol.,* 1927-28, **11**, 803-821.
9. ———. Geotropic orientation in arthropods: II. *Tetraopes. J. Gen. Physiol.,* 1928-29, **12**, 675-693.
10. Hunter, W. S. The behavior of the white rat on inclined planes. *Ped. Sem.,* 1927, **34**, 299-332.
11. Keeler, C. The geotropic reaction of rodless mice in light and in darkness. *J. Gen. Physiol.,* 1927-28, **11**, 361-368.
12. Kropp, B. Geotropic orientation in arthropods: IV. The beetle *Pyrophorus. J. Gen. Psychol.* (in press).
13. Kropp, B., & Crozier, W. J. Geotropic orientation in arthropods: III. The fiddler crab *Uca. J. Gen. Physiol.,* 1928-29, **12**, 111-222.
14. Piéron, H. Gravitational sensitivity and geotropic reactions in slugs. *J. Genet. Psychol.,* 1928, **35**, 3-17.
15. Pincus, G. Geotropic creeping of young rats. *J. Gen. Physiol.,* 1926-27, **10**, 525-532.
16. Wolf, E. Geotropism of *Agriolimax. J. Gen. Physiol.,* 1926-27, **10**, 757-765.


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## L'ORIENTATION GÉOTROPIQUE DES POUSSINS
(Résumé)

L'importance des méchanismes proprioceptifs comprenant le ton différentiel des muscles des deux côtés du corps en déterminant l'étendue de l'orientation géotropique chez les limaces, les rats et les souris, les scarabées, les chenilles et les écrevisses a été accentuée par les expériences de Crozier et de ses collaborateurs. Ces travailleurs ont employé aussi ces expériences géotropiques comme moyen d'analyser les procédés nerveux centraux.

Le travail par Hunter avec les rats et par Piéron avec les limaces a tendu à appuyer sur l'oreille comme spécialement déterminative dans les réponses géotropiques plutôt que les méchanismes suggérés par Crozier pour la limitation de l'orientation.

L'auteur a examiné l'orientation géotropique des poussins comme animaux (bipèdes) différant beaucoup de ceux employés auparavant à l'égard de leur mode de progression. L'orientation des poussins sur une surface plane inclinée montre un rapport linéaire entre le degré de l'orientation en haut sur la surface plane et l'angle de la surface plane. On peut expliquer ce rapport en termes de la posture du poussin en marchant sur la surface plane, ce qui cause la tension de certains muscles et l'excitation subséquente des impulsions proprioceptives.

On appuie sur l'importance de l'oreille en maintenant la posture de laquelle dépend la limitation proprioceptive de l'orientation.

HOAGLAND

## GEOTROPISCHE ORIENTATION BEI KÜCKEN
(Referat)

Die Bedeutung des propriozeptiven Mechanismus, der den unterschiedlichen Tonus der Muskeln beider Seiten des Körpers einschliesst, um die Ausdehnung der geotropischen Orientation bei Schnecken, Ratten und Mäusen, Käfern, Raupen und Krebsen zu ermitteln, ist kürzlich durch die Experimente von Crozier und seinen Mitarbeitern besonders betont worden. Dieselben Forscher haben auch geotropische Experimente zur Analyse der Vorgänge im Zentralnerven benutzt.

Versuche von Hunter mit Ratten, und von Piéron mit Schnecken, deuten auf das Gehör als hauphsächlich bestimmend in geotropischen Reaktionen, vielmehr als der von Crozier angedeutete Mechanismus.

Der Verfasser untersuchte die geotropische Orientation kleiner Kücken als Tiere (Zweifüssler) die in ihrer Art der Fortbewegung sehr verschieden von den früher benutzten Tieren sind. Die Orientation der Kücken auf einer schiefen Fläche erwies eine lineare Beziehung zwischen dem Grad der aufwärts führenden Orientation auf der Fläche und dem Elevationswinkel derselben. Diese Beziehung kann nach der Haltung der Kücken, beurteilt werden wenn sie auf der Fläche einherspazieren wobei gewisse Muskeln gestreckt werden somit propriozeptive Impulse auslösend.

Man betont die Bedeutung des Gehörs zur Beibehaltung der Stellung wovon die propriozeptive Stimulation abhängt.

HOAGLAND

## ГЕОТРОПИЧЕСКАЯ ОРИЕНТАЦИЯ У ЦЫПЛЯТ.

(Реферат).

Важность проприоцептивных механизмов, включая сюда различный тонус мышц на обоих сторонах тела, в определении размера геотропической ориентации у некоторых моллюсков, крыс и мышей, жуков, гусениц и крабов была недавно установлена опытами Крозье (Krozier) и его сотрудников. Опыты по геотропизму были также использованы этими учеными, как средство для анализа процессов в центральной нервной системе.

Исследования Гёнтера (Hunter) над крысами и Пиерона (Piéron) над моллюсками стремились показать, что при ориентации ухо прежде всего определяет геотропическую реакцию,—скорее, нежели механизмы, на которые указывает Крозье.

Автор исследовал геотропическую ориентацию у маленьких цыплят, как животных (двуногих), резко отличающихся от ранее исследованных способом передвижения. Ориентация цыплят на наклонной плоскости обнаружила прямое отношение между степенью передвижения вверх по доске и углом наклона доски. Это отношение может быть об'яснено положением тела цыпленка во время передвижения по доске, которое определяется напряжением известных мышц и последовательностью возбуждений со стороны воспринимаемых импульсов.

Значение уха в сохранении положения тела, от которого зависит принимаемая ориентация, доказана.

**Хогленд (Hoagland).**

# A QUANTITATIVE THEORY OF FEELING*

*From the Psychological Laboratories of Clark University*

JOHN PAUL NAFE

## PRESSURE-PAIN

During the years 1926 and 1927 E. D. Adrian (1, 2) and E. D. Adrian and Y. Zotterman (4, 5) published a series of papers upon records of afferent impulses. The facts as shown by the published records are of great importance to our theories of felt experience and, although the authors have contented themselves with an interpretation in terms of the current theory of specific nerve energies, the facts themselves demand no such treatment but lend themselves more readily to another explanation. Specifically the facts appear to show a direct relation between (1) the intensity of stimulation; (2) the frequency of impulses; and (3) the pressure-pain series (intensity) of felt experience.[1]

Interpretation of the records is often difficult because the number of fibers involved is unknown. Adrian and Zotterman have shown conclusively, however, in working with impulses aroused by *a single receptor* within a muscle, that an increase in the intensity of stimulation is accompanied by an increase in the frequency of impulses. This is in accordance with all other pertinent findings and we must generalize from this one *known* relation that an increase of stimulation results in an increased frequency of impulses. The conclusion is subject to any evidence which may be produced tending to show exceptions but it is a presumption which we cannot dismiss in any instance without direct proof to the contrary. The evidence for this correlation is unquestioned if we accept the doctrine of specific energies, that is, for pain it is true and for pressure it is true, but what has been generally accepted without sufficient factual evidence is the assumption of "pain nerves" and "pressure-nerves."

In other experiments in this series, Adrian and Zotterman found very positive evidence for a longer adaptation time for more inten-

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[1]We cannot here reproduce the findings of these experiments. The present paper presuposes a familiarity with the papers in question or with the re-presentation offered in (3).

sive stimuli, other things being equal. No evidence is found for any other differences between the records for "painful" and "non-painful" stimuli. There is no difference shown in the duration of individual impulses or in the grouping of impulses and, under their interpretation, based however upon the probable number of "pressure receptors" and "pain receptors," no positive evidence of significant differences in frequency of impulses. In this last conclusion the authors are placing the dictum of Müller above their own experimental findings cited above, which raise a presumption of an increase in frequency of impulses with increased stimulation unless there is positive evidence to the contrary.

The difficulties of interpretation, then, consist in the main of two elements: (1) The fact that in most of the experiments the number of fibers is not known. Hence an increase in the record of frequency of impulses may be due to an increase in the number of fibers activated or may be due to an increased frequency in the individual fibers or both. (2) In cases where a change is made from "painful" to "non-painful" stimuli, showing a correlated variation in the frequency of impulses, it may always be maintained, without possibility of proof to the contrary in the individual record, that the difference in frequency is due to the excitation of entirely different sets of receptors. We may attack the theory involved in this second difficulty in other ways and so possibly eliminate it. The theory of specialized receptors was first offered by von Frey (15) without factual support other than the existence of areas of sensitivity and the histologically differentiated receptors. The theory is not obviously true and demands strong factual support to justify the general acceptance it has received. On the negative side, extirpation experiments have furnished conclusive proof that the supposed relation does not exist (12).

There is a further confusion in the interpretation of the records of events of the first moment of stimulation. This is due to the fact that we have no adequate theory of stimulation and appears as a tendency to equality in the frequencies regardless of the nature of stimulation. Events occur in the first moment of contact between living tissue and a foreign body which may account for all such similarities in the records, and, if we exclude this first moment as equivocal, there is little left in the records of these experiments to show an approximate identity in the frequencies of discharge as between painful and non-painful stimulation. The frequencies in the actual instances given do not vary greatly, but the differences in stimulation

also are not great. A needle run into the finger 1 mm. in 20 sec. arouses a decided prick but it is not extremely painful. At such a rate one may continue to run the needle clear through the tissue without causing any very great pain.

In regard to the first-mentioned difficulty, i.e., the number of fibers involved, while we are forced for the present to fall back upon a negative line of evidence, it is significant that stimulation of a preparation with "painful stimuli," where the record is clear, usually results in a higher frequency, and no such record shows a decreased frequency. So the stimulation of the tissues of a frog with a needle, weighted in one series with 43 and in another with 4 gms., except for the first moment, .05 sec., shows that the less intensive stimulus consistently produces a less frequent rate of impulses, and, as shown, the conditions of stimulation appear to be comparable (2, p. 47).

The connection is again brought out in another experiment reported in this series (1, p. 65 ff.). In this case the limb of the preparation (cat) containing the part of the skin under observation rested upon a table. The record of the preparation at rest showed a rather rapid series of oscillations due, presumably, according to Adrian, to the pressure of the leg on the table and the movement of hairs by air currents. These oscillations were found to increase in frequency and amplitude when the skin was nipped by a pair of forceps or was pricked by a pin. "The increase in frequency may be relatively small if the frequency is already high in the resting limb. . . . [but] . . . there is no indication that the additional impulses set up by a painful stimulus differ much from those with the limb undisturbed which are presumably due to non-painful stimuli."

Psychological evidence for the same triple relationship has been cited in a previous article (17). In this case the evidence is more equivocal, as it may be interpreted in terms of the current theory, although, as there pointed out, the facts permit a theory more simple than that formulated under the theory of specific nerve energies and, if the facts permit a more simple theory, economy demands it.

The following quantitative theory is offered to cover the facts as shown in both the neurological and psychological researches.

The sense of feeling includes the psychological experience accompanying the activity of all the receptors of the body except those for the special senses of vision, audition, olfaction, and gustation, and possibly those of the non-acoustic labyrinths concerned with equi-

librium, etc. Different feelings such as pressure, fear, pain, tickle, warm, etc., are experienced, but no such experiences have been shown to be either "simple" or "qualities" in the sense that they are mediated by separate neural systems with specialized receptors and cortical areas. Not only has such a state not been proved, it has no factual basis at all and cannot be reconciled with the best evidence as it exists.

The "series of nervous discharges" with which we are here dealing may be considered as functional units, i.e., each group of discharges represents a separate occurrence. The only objective elements involved in such occurrences consist of afferent nervous impulses which, for our purpose, may be considered to be undifferentiated, varying only in number and in their temporal and spatial relations with each other. These variable temporal and spatial relations are found in:

1) The frequency of impulses.
2) The length of time the impulses continue.
3) The area over which the impulses arise.
4) The relative number of fibers activated within such an area.[2]

The total number of impulses is involved in each of these relations, the first and second uniting to determine the number of impulses which will arrive over the individual fibers and the third and fourth uniting to determine the number of fibers activated. Consequently these aspects of the whole occurrence are a measure of the total number of discharges, and variation in any one or more will bear directly upon the total amount. On the psychological side we are entitled to posit a possible difference in conscious experience for each possible variation in the manner of discharge.

Variation in the frequency of impulses is correlated with changes in felt intensity, variation in duration with the duration of experience, variation in the area stimulated with felt extensity, and variation in the number of fibers activated within such an area is correlated with the felt density.

Between actual occurrences, variation in the *intensity* of stimulation will be accompanied by variation, in the same direction, of all four variables but variation in the *form* of stimulation will vary the

[2]This list is over-simplified but justifiably so, perhaps, because of our limited knowledge in regard to certain other possible variations and their functional importance.

four independently of each other, i.e., a change in the size of the stimulus will affect only felt extensity.

This completes the list of possible variations, neurological and psychological, and ends our analysis. We now attempt to set forth the manner in which ordinary experiences occur.

Stimulus objects or conditions may be, as is often stated, mechanical, chemical, thermal, or electrical, but the action of such environmental conditions is not immediate. Their occurrence is accompanied by changed conditions within the tissues, wherein the conditions within the receptors vary from those of their environment. In the consequent changes toward a re-establishment of equilibrium, impulses are aroused in the corresponding nerve fibers. We know little about single impulses or the activity of single fibers. Stimulation ordinarily results in the discharge of a group of receptors, which may be regarded as a unit occurrence. Particular experiences, e.g., wet, cold, or pressure, depend, for their similarities and differences, upon the "pattern" or "arrangement" of these discharges as a whole, i.e., the number of impulses involved and their temporal and spatial arrangement. Similar stimulus conditions will, from time to time, arouse similar conditions within the tissues, and these conditions will be accompanied by similar psychological experiences. The fact that some experiences will recur with sufficient frequency to be named indicates nothing as to their ultimate simplicity, nor will the ability or inability of an observer to analyze such an experience be any real proof as to the neurological mechanism by which it is aroused.

Further complications of experience are occasioned by the reflex activities aroused, either with felt experiences or with experiences aroused in other sense departments. Such activities are of two very different sorts:

1) Activity of muscular and glandular systems which is followed by the arousal of afferent impulses and consequent felt experience. When an object comes into contact with the skin one may see, under a binocular microscope, changes occurring within the tissues. These visible changes may consist of movement of skin and dilation of superficial capillaries, the red blood showing near the surface and, if the contacts are repeated to the extent of chafing, the changes become apparent without magnification as inflammation and change in texture of the skin. These activities, however, are not limited to the small changes which occur near the point of stimulation but may in-

clude shudders, chills, thrills, laughter, twitching, weeping, quivers, nausea, changes in tonus, etc., or even complicated, coordinated activity of muscle systems. Such reflexly aroused activities as these in turn arouse afferent impulses and consequent feelings which, in less intensive experiences, tend to modify the existing pattern of experience but, in more intensive experiences, tend to be singled out as an affective or emotional or other accompaniment of the more directly aroused part of the experience.

2) Consciousness itself is reflexly aroused. The more intensive a stimulation the more likely is it to be accompanied by a conscious experience. Series of impulses of low frequency are not likely to be accompanied by consciousness at all.

Each of these reflexly aroused experiences modifies the existing experience, and the number and vigor of such reflexes, aroused with felt experience, vary with the intensity and form of stimulation. Regardless of the number and amount of its complications, the neural excitation remains a temporal and spatial "arrangement" or "pattern" of a number of impulses and as such may be accompanied by a related conscious experience. In all these complications of "patterns of discharges" there are no new elements. Familiar patterns are named, and, if an experience arises for which we haven't names enough, we supplement our description with qualifying adjectives. After some unusual experience, such as a surgical operation, we may even launch into a full description, but all the terms used, descriptive of felt experience, imply variations of patterns and are described in terms of intensity, density, extensity, duration, and localization, or collocations of such aspects within the experience as a whole.

## Localization

That localization is learned may be taken as fairly well established (14, 16, 18, 19, 20). This defeats the theory of "local sign" which, as a quality of the experience, is directly experienced. The only neurological principle which then fits the facts of localization is a correlation of localization with the cortical locus of the central termination of the particular fiber or fibers stimulated (the correlate of "qualities" under the older theory). There is no conscious experience of such locus but it is a well-known principle of learning that conditioning may take place to a stimulus which does not itself rise to consciousness.

## Sensitivity to Temperature Differences

It is necessary to bear in mind the fact that we do not feel either physical warmth or cold; language habits sometimes color our thinking. Changes in temperature are accompanied by changes within the tissues, and it is with the consequent "discharges" of receptors and the aroused nervous impulses that we correlate our sensitivity to all felt experiences. The only thing *required* to distinguish warmth or cold from other experiences or from each other is some characteristic difference in the aroused pattern.

Knowing that the felt intensity differences are correlated with the rate of impulses, we are not surprised to find that one cannot set up the pattern for pressure and pain in the same place at the same time or that the same is true for hot and warm and for cold and cool. But crosswise we may combine. We may experience warm pressures, cool pressures, cold pains, and hot pains, but warmth and cool will not occur together in the same place, nor will hot and cold. This is evidence of some functional opposition in the requisite conditions for experiences of warmth and cold.

We do not know the basis for this opposition but we may suppose, only by way of illustration, that the characteristic element in the pattern for warmth lies in the impulses aroused by relaxation of some muscle units and that cold is characterized by the impulses aroused by contraction of the same muscle units. Obviously the same muscle cannot both contract and relax at the same time.

The experience of warmth can be aroused only between temperatures slightly above the indifference point and some 12°C. above that point. Higher temperatures produce a pattern of prick and pressure experiences known as "heat" and, contrary to the usual statement of the case, the transition is not particularly abrupt (17). At the lowest temperatures warmth merges into thermally indifferent pressure so gradually that one cannot tell when one begins and the other ceases. As the warmth becomes more intense, it gradually brightens up. Fine bright points begin to occur, and these increase in density and brightness as the temperature rises. At very high temperatures (for feeling) the experience solidifies into a bright, solid, pain.

Almost the same words might be used to describe the course of cool and cold experiences although they are distinct. Cool merges into pressure at the indifference point but, as the temperature drops, the bright points become discrete and brighter much more quickly

than with warmth experiences. At some 12°C. below the indifference point the bright points become massive, and at very low temperatures cold also merges into pain.

At their extremes, hot and cold experiences are identical and neither retains any vestige of the distinguishing features of warm and cool. With punctiform stimuli an observer can distinguish them from the prick of a needle only because of the pressure element in the latter. Under ordinary circumstances there are other sensory cues present so that one knows whether he is dealing with hot or cold objects.

Von Frey (15) reports that with warm, punctiform stimulation (about 45°C.) he would *occasionally* arouse an experience of cold. "Paradoxical cold," as such an experience is called, has been verified by Boring and others (8). Von Frey states that when he gets such reports, stimulation is usually not at a cold spot but near one.

Following this work, Alrutz (6) found that the simultaneous stimulation of warm and cold spots, if they are not too far apart, sets up an experience of "heat," and this has been generally demonstrated by Cutoto's "heat-grill"[3] (10, 7, 9).

The characteristic thing about cold being the discrete bright points, the characteristic aspect of warmth being its resemblance to pressure, and the experience of heat being a series of discrete pricks in a mass of pressury experience, we might have expected that the experience resulting from setting up warm and cold experiences near each other would resemble heat. Observers agree that the experience is that of heat except that it is not followed by pain and except that an occasional warmth and an occasional cold are felt, and that, when such experiences drift in for the moment, heat disappears from the position where the warm or the cold is felt.

Under the quantitative theory this phenomenon is explained as follows: The increments of experience necessary to mark warmth and cold experiences being functionally opposed, the close proximity of adequate stimuli for both prevents either pattern from being formed, and the experience will continue without such modifications. Thus we have prick or bright points without the "tag" for cold and a body of pressure without the "tag" for warmth. That the condition for setting up these increments is present, but held in balance,

[3]The grill consists of a series of small pipes running, alternately, with cold and warm water.

is shown by the fact that occasional shifts permit the introduction of momentary experiences of warmth and cold.

The older qualitative theory accounts for these facts in quite another way. The impressions of warmth and cold are mediated by separate receptors and fibers. Simultaneous stimulation of the two creates an experience of heat. This is possible under ordinary circumstances because a stimulus of such temperatures will arouse the warmth response and the paradoxical cold response, and the two will fuse to form heat experiences. This explanation is vital to their theory.

To determine the fact we performed the following experiment: Upon the end of a copper soldering-iron a point was filed. It was flat at the contact end and, in the first series of experiments, 1 mm. in diameter across the base, in the second series of experiments, 1/2 mm. in diameter, and in the third something less than 1/2 mm. The method of heating the iron was by water bath ranging from 45° to 80°C. The iron was taken from the bath, wiped dry, and suspended by a heavy rubber band to a framework so that it hung point downward and could be further lowered and manipulated by hand. All temperatures recorded were temperatures of the bath at the time the iron was removed and, with the higher temperatures used, there was undoubtedly some loss. The iron was used for three minutes after removing it from the bath. Only one of three observers knew the nature of the problem or the systematic significance of the reports.

In a control series the bath was at 33°C. This series of tests was taken to determine the nature of the reports independently of temperature effects. There were no reports of prick, pain, or heat. The control series was repeated every time the size of the point was reduced. A series of tests was then made, the parts of the skin used being varied to avoid complications with after-effects of previous trials, stimulation being effected by bringing the heated point into contact with the skin of the observer.

According to the quantitative theory, the reports should have been in terms of prick, pain, and heat. According to the older, qualitative theory, the reports should have been in terms of warm and cold. There were a few reports of warm, usually as a complication of other experience. There were no reports of cold. Over 90% of all the reports involved nothing but prick, pain, and heat. With the stimulus from the bath at 70°C. the prick of a needle was sometimes substi-

tuted for the hot point. There were at first no changes in the reports but subsequently there appeared a tendency to designate the experience aroused by the needle as a "heavy prick." No other distinction was made.

It is evident that the application of stimuli of high temperatures is consistently accompanied by neither experiences of warmth nor of cold.

The qualitative theory of warm and cold has its roots deeply in the whole matter of specialized receptors, and there is little extant evidence to support it. The assumed correlate for cold is the Krause end-bulb, and that for warmth has varied from one organ to another. Extirpation experiments have always failed to discover any such relation. Dallenbach (12) has recently reported an exhaustive study of this matter, again by extirpation. His results are convincing. The tissue beneath large numbers of cold and warm spots were examined histologically, and there was not a single specialized ending of any kind found within the whole area of tissue taken from three observers.

It has been reported that in the cornea of the eye, Krause's end-bulbs and free nerve-endings are the only terminal nerve structures found. This, coupled with the report, which has not been verified, that the cornea of the eye is sensitive only to cold and pain, led many to look for a relation between the two. The laboratories in which many of these investigations were carried on are working under a theory that with punctiform stimuli they succeed in stimulating single receptors. The result is that they stimulate the eye with points, and their findings may mean only that the eye is extremely sensitive. Our observers, stimulated upon the cornea of the eye with a loose strand of cotton, reported contact and pressure experiences.

We must conclude that sensitivity to temperature differences consists of two intensive series, one running from thermally indifferent pressure through warmth and heat to thermally indifferent pain; and the other running from thermally indifferent pressure through cool and cold to thermally indifferent pain; that experiences of warmth and cold are modifications only of general patterns of felt experience (13) and that the physiological conditions requisite for these two experiences are functionally opposed; and that temperature changes are accompanied by those conditions within the tissues correlated with discharge of the receptors, and discharge will occur with

consequent experience whether or not the physiological conditions correlated with the characteristic differences (warmth and cold) are present.

In view of the unsettled condition of psychological theory in general, the acceptance of the above theory forces a generalization of its principles. We restate Müller's fundamental law of psychology as follows:

The specific accompaniment of sensory excitation is correlated with the number of nervous impulses and their temporal and spatial relations.

Specifically it raises a presumption against all psychological and neurological theories based upon individual receptor differences or upon differences in experience due to cortical locus. The concept of "quality" in its present form must be abandoned.

Helmholtz' concept of modality must be examined very closely; it is a similar doctrine. It should be abandoned upon general principles for the reason that it limits possible theories without a single fact for itself to stand upon. It is sheer dictum. Moreover, there is already some positive evidence against it.

## References


1. Adrian, E. D. The impulses produced by sensory nerve endings: Part 1. *J. Physiol.*, 1926, **61**, 49-72.
2. ————. The impulses produced by sensory nerve endings: Part 4. *J. Physiol.*, 1926-7, **62**, 33-51.
3. ————. The basis of sensation. New York: Norton, 1928. Pp. 122.
4. Adrian, E. D., & Zotterman, Y. The impulses produced by sensory nerve endings: Part 2. *J. Physiol.*, 1926, **61**, 151-171.
5. ————. The impulses produced by sensory nerve endings: Part 3. *J. Physiol.*, 1926, **61**, 465-483.
6. Alrutz, S. On the temperature-senses. *Mind*, 1898, **7**, 141-144.
7. Alston, J. H. The spatial condition of the fusion of warmth and cold in heat. *Amer. J. Psychol.*, 1920, **31**, 303-312.
8. Boring, E. G. Sensations of the alimentary canal. *Amer. J. Psychol.*, 1915, **26**, 1-56
9. Burnett, N. G., & Dallenbach, K. M. The experience of heat. *Amer. J. Psychol.*, 1927, **38**, 418-431.
10. Cutolo, F. Jr. A preliminary study of the psychology of heat. *Amer. J. Psychol.*, 1918, **29**, 442-448.
11. Dallenbach, K. M. A bibliography of the attempts to identify the functional end-organs of cold and warmth. *Amer. J. Psychol.*, 1929, **41**, 344.
12. ————. The temperature spots and end-organs. *Amer. J. Psychol.*, 1927, **39**, 402-427.
13. Dimmick, F. L. On the localization of pure warmth sensations. *Amer. J. Psychol.*, 1915, **26**, 142-150.

14. FRANZ, S. I. Accuracy of localization of touch stimuli on different bodily segments. *Psychol. Rev.*, 1913, **20**, 107-128.

15. VON FREY, M. Beiträge zur Sinnesphysiologie der Haut. *Ber. d. kgl. sächs Ges. d. Wiss., Leipzig, math-phys. Kl.*, March 4, 1895, 172.

16. MEYER, M. F. The fundamental laws of human behavior. Boston: Badger, 1911.

17. NAFE, J. P. Psychology of felt experience. *Amer. J. Psychol.*, 1927, **39**, 367-389.

18. PETERSON, J. Local signs as orientation tendencies. *Psychol. Rev.*, 1926, **33**, 218-236.

19. RENSHAW, S. The errors of cutaneous localization and the effect of practice on the localizing movement in children and adults. To appear in *J. Gen. Psychol.*, 1930, **3**.

20. STRATTON, G. M. Vision without inversion of the retinal image. *Psychol., Rev.*, 1897, **4**, 341-360; 463-481.

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## UNE THÉORIE QUANTITATIVE DES SENTIMENTS

(Résumé)

Le "sens du sentiment" est défini pour comprendre toute l'expérience "sentie," l'expérience affective et émotive non exceptées, et une théorie néuralogique est offerte pour le sens qui ne comprend pas le concept de "qualité."

On fait la corrélation de pression-douleur avec l'intensité et les autres variables sont la durée, la densité, et l'extension. Chaud-froid varie d'une manière caractéristique des formes non-thermales de l'expérience mais ne comprend pas de différence fondamentale. La localisation est un processus appris dépendent du locus cortical.

Nulle expérience n'est simple. Des objets semblables de stimulus, de temps en temps, établissent de semblables conditions dans les tissus et celles-ci sont accompagnées de semblables expériences psychologiques. Ainsi on généralise et nomme les types les plus fréquents.

Les expériences sont plus compliquées par les activités réflexes établies, surtout les éléments effectifs et émotifs de l'expérience.

L'évidence psychologique donnée est complétée par des citations d'un article antérieur et on cite des observations néuralogiques de l'oeuvre de E. D. Adrian et d'autres.

On offre une nouvelle formule de la loi générale pour remplacer les "énergies spécifiques des nerfs" de Müller.

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## EINE QUANTITATIVE THEORIE DES GEFÜHLS

(Referat)

Der Gefühlssinn wird beschrieben als alle gefühlte Erfahurung umfassend sogar Affektion und Gefühlserregung; dazu wird eine neurologische Theorie für den Sinn aufgestellt, welcher den Begriff der Qualität miteinschliesst.

Druckschmerz wird mit Intensität korreliert, und die andern Variablen sind Dauer, Dichtigkeit und Ausdehnung. Warm-kalt unterscheidet sich in charakteristischer Weise von den nichtthermalen Mustern der Erfahrung, weist jedoch keine wichitigen Unterschiede auf. Lokalisation ist ein erlernter Vorgang, der von dem kortikalen Lokus abhängt.

Keine Erfahrung ist einfach. Ähnliche Reizmittel erzeugen zeitweise

ähnliche Zustände in den Geweben, und werden von ähnlichen psychologischen Erfahrungen begleitet. Daher die Verallgemeinerung und die Benennungen der häufigern Typen.

Erfahrungen werden ferner durch Reflextätigkeiten komplizirt, besonders durch die affektiven, und die Erregungselemente der Erfahurung.

Psychologische Beweise werden durch Hinweise auf eine frühere Abhandlung ergänzt, und neurologische Beweise werden den Arbeiten von E. D. Adrian und andern entnommen.

Eine neue Fassung des allgemeinen Gestzes wird anstatt Müller's "specific nerve energies" (Spezifiscle Nervenenergie) vorgeschlagen.

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## КОЛИЧЕСТВЕННАЯ ТЕОРИЯ ЧУВСТВ.

(Реферат).

Под „чувствованиями" в данном исследовании понимается весь чувственный опыт, включающий эмоции и аффекты. Автор дает здесь попытку неврологической теории чувствований, не включающей понятия „качества".

Так, ощущение боли связывается с интенсивностью; другими факторами является здесь продолжительность, насыщенность и экстенсивность. Ощущения тепла и холода отличаются от не термических ощущений, но не представляют каких-нибудь принципиальных особенностей. Локализация является результатом известного приобретенного процесса, зависящего от размещения возбуждений в коре.

Ни один опыт не является „простым". Одинаковые стимулы время от времени вызывают сходные изменения в тканях организма, а эти изменения сопровождаются соответствующими сходными психологическими состояниями. Отсюда возникает возможность определения и классификации наиболее часто встречающихся типов.

Опыт в дальнейшем усложняется рефлекторной активностью (особенно в случае эмоциональных и аффективных процессов).

Психологические данные этого исследования дополняются данными неврологических наблюдений Э. Д. Адриана и др. Автор формулирует здесь общий закон, который должен заменить Мюллеровское положение о „специфичности нервной энергии"

**Нэйф (Nafe).**

# DISCRIMINATION OF HOMOPHENOUS WORDS BY TACTUAL SIGNS*

*From the Psychological Laboratories of Northwestern University*[1]

ROBERT H. GAULT

"Homophenous words" are those that look alike to the lip-reader. *Grand* and *grant* are illustrations. There are upwards of 2,000 such words in the English language. They occur in groups of *two,* as in the example above, and in groups of *three, four, five,* and more. If the words in a given group are truly homophenous and are normally spoken, the lip-reader cannot discriminate among them when they are isolated from sentence structure. When they enter into the composition of spoken sentences, the lip-reader may get their meaning but only because he jumps at it from the context.

Can deaf subjects distinguish homophenous words by their tactual signs via the "teletactor" (single-unit receiver)?

In an attempt to answer this question, we selected the groups of homophenous words that are found below. They were found in Dr. Nitchie's list as published in the *Association Review* for February, April, and June, 1903. Because it was impracticable to employ the entire list of approximately 2,000, we made random choices of groups of *two,* of *three,* etc., a total of 102 groups, and those we have taken as the basis for our study. The groups include 68 of two words each; ten of three each; six of four; six of five; five of six; four of seven; one of eight; one of nine; and one of ten words. This makes a grand total of 305 words.

## THE SUBJECTS

H—An intelligent man of 35 years of age; an instructor in mathematics in a school for the deaf. Good lip-reader. Deafened at the age of six years. Audiometric record, 0-10 (loss of 100-90). Hours of laboratory experience prior to this work, 90.

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[1]The continuation of work in this Laboratory is made possible by the aid of a grant from the Carnegie Corporation to the Carnegie Institution of Washington. Instruments for the purpose have been furnished by Bell Telephone Laboratories of New York City.

B—Young man, student; age, 27. Poor lip-reader. Deafened at five years of age. Audiometric record, 5-5. Hours of laboratory experience prior to this work, 132.

F—Young man, student; age, 22. Good lip-reader. Deafened at five years of age. Audiometric record, 45-15. Hours of laboratory experience prior to this work, 25.

A—Young woman, typist; age, 28. Good lip-reader. Deafened at age of 12. Audiometric record, 25-20. Hours of laboratory experience prior to this work, 15.

### Instrumentation and Method

The "teletactor" was used as a means for communicating the tactual stimuli to the fingers of the subjects. This consists of a microphone at the speaker's mouth, a 7 A Western Electric Amplifier, a six-volt storage battery, a B battery or an eliminator, and a single-unit receiver analogous to the ear-piece of a radio set. The latter is held in the fingers of the subject. (Four subjects and four receivers were used simultaneously in this experiment.) The subject's thumb or index finger is held lightly against the vibrator or diaphragm. In this manner *speach is felt.*

A group of words such as *grand-grant* was written upon the blackboard. The words were pronounced five times in a known order, while the experimenter (G) was in full view of the subjects. After this preliminary exercise the speaker, without reference to the blackboard, pronounced the same words in unknown order, 40 times each group, in series of ten. The subjects had no contact with the receiver, and the microphone was not at the mouth of the speaker. His face was fully exposed. The subjects were asked to write the words upon their pads if they could recognize them—otherwise to make a dash. There was no check-up and no stop till the whole series was completed. That is a partial test of lip-reading ability.

This was followed by pronouncing the same words in known order into the microphone while the *speaker was out of sight in a neighboring room.* In this situation each subject was holding his receiver in his fingers. In the course of five pronunciations of the group, an assistant with the subjects always pointed to the word upon the blackboard as the experimenter pronounced it. The subjects had already become somewhat acquainted with differences in feel among certain consonants (differences of abruptness and of "weight" and "sharpness," as they said. H, B, and F at this time, too, were

somewhat familiar with the feel of six vowel qualities. At this time H had already had 90 hours of laboratory experience; B, 132 hours; F, 25 hours; while A had had only 15 hours of experience.

As soon as these five readings in known order had been completed, the experimenter, hidden in the next room, proceeded to pronounce the words in unknown order—each group 40 times. The subjects attempted to write upon their pads as before.

In relation to the second group of words, the *Touch* work was done first and the *Lip-reading* second. Thus we alternated, from first to last, for the purpose of neutralizing whatever advantage there might be in a temporal arrangement.

After every 40 readings, we checked up.

## Stimuli and Results

The groups of words we used in this piece of work are on the following pages and together with them are the scores (per cent) that each subject made by each method of reception.

It is worth the remark here that if the words in every group below are truly homophenous, the lip-reader should not be able to recognize them more frequently than chance should lead us to expect. In a number of instances—as the figures opposite L indicate—the result is above chance. This fact tends to load the dice against the possibility that touch can show an advantage.

It is not clear why, in certain instances, the lip-reader gave a result considerably above chance. No one of the subjects can hear the human voice through the air. It is possible that the experimenter, without his knowledge, has some peculiarities of facial expression, while speaking, that the subjects were able to make good use of.

An inspection of Table 1 makes it clearly a general rule that the T score is higher than the L score.

Averages for the groups in Table 1 are of little, if any consequence because of great differences among the subjects. A had not much more than begun acting as laboratory subject when this piece of work was undertaken. F far surpasses every other one in the group as a lip-reader. (This statement is based upon common observation. At the time this experiment was going on, no objective test. worthy of the name, was available.) B is very temperamental. However, one who is interested in speculating whether a class as a whole will get on better or worse with the addition of tactual to visual cues in connection with the interpretation of speech will be interested in the fact that there are only eight of the total 102

TABLE 1

PERCENTAGE OF IDENTIFICATION BY TOUCH AND BY LIP -READING, RESPECTIVELY, OF EVERY WORD IN 102 GROUPS BY EVERY SUBJECT

Note: In all tables, figures following T indicate the success of subjects at making identification *by touch alone without vision of the speaker's face.* Figures following L, correspondingly, indicate success at *straight lip-reading without feeling the speaker's words.*

| Stimuli | | H | B | F | A | Av. for all subjects |
|---|---|---|---|---|---|---|
| aim | T | 100 | | | 100 | 100 |
| ape | L | 90 | | | 70 | 80 |
| arch | T | 100 | 90 | 100 | 100 | 98 |
| harsh | L | 70 | 60 | 40 | 60 | 58 |
| bake | T | 100 | 90 | | 40 | 77 |
| make | L | 60 | 15 | | 50 | 42 |
| beam | T | 100 | 95 | | 100 | 98 |
| peep | L | 90 | 70 | | 70 | 77 |
| bribe | T | 90 | 95 | | 80 | 88 |
| prime | L | 50 | 60 | | 50 | 53 |
| brown | T | 95 | 90 | | 95 | 93 |
| proud | L | 80 | 45 | | 65 | 63 |
| balm | T | 80 | 30 | 90 | 60 | 65 |
| palm | L | 40 | 0 | 70 | 40 | 38 |
| best | T | 70 | 60 | 40 | 30 | 50 |
| pest | L | 50 | 50 | 30 | 60 | 48 |
| blood | T | 90 | 60 | 100 | 80 | 83 |
| blunt | L | 60 | 40 | 60 | 60 | 55 |
| blouse | T | 80 | 90 | 100 | 100 | 93 |
| plows | L | 50 | 40 | 100 | 70 | 65 |
| brace | T | 70 | 60 | 70 | 60 | 65 |
| prank | L | 80 | 80 | 90 | 30 | 70 |
| bellow | T | 100 | 80 | 80 | 90 | 88 |
| mellow | L | 70 | 100 | 30 | 70 | 68 |
| beseech | T | 90 | 50 | 60 | 100 | 75 |
| besiege | L | 40 | 30 | 30 | 0 | 25 |
| button | T | 90 | | | 55 | 73 |
| mutton | L | 55 | | | 50 | 53 |

| Stimuli | | H | B | F | A | Av. for all subjects |
|---|---|---|---|---|---|---|
| butter | T | 85 | | | 65 | 75 |
| mutter | L | 40 | | | 35 | 38 |
| charm | T | 95 | | | 100 | 98 |
| sharp | L | 65 | | | 80 | 73 |
| choice | T | 100 | 75 | 100 | 85 | 90 |
| joys | L | 55 | 65 | 85 | 85 | 73 |
| chum | T | 100 | 100 | 80 | 95 | 94 |
| jump | L | 95 | 75 | 75 | 75 | 80 |
| clock | T | 90 | 95 | 90 | 70 | 86 |
| clog | L | 75 | 80 | 65 | 50 | 68 |
| cream | T | 90 | 100 | 100 | 100 | 98 |
| creep | L | 90 | 90 | 100 | 60 | 85 |
| crumb | T | 90 | 100 | 80 | 100 | 93 |
| crub | L | 55 | 55 | 55 | 55 | 55 |
| chair | T | 80 | 70 | 50 | 20 | 55 |
| share | L | 60 | 70 | 20 | 30 | 45 |
| chafe | T | 70 | 40 | 100 | 90 | 75 |
| shave | L | 80 | 90 | 80 | 60 | 78 |
| crease | T | 100 | 30 | 100 | 100 | 83 |
| grease | L | 60 | 20 | 50 | 60 | 50 |
| cried | T | 70 | 70 | 90 | 70 | 75 |
| grind | L | 30 | 60 | 60 | 80 | 58 |
| dive | T | 30 | 80 | 100 | 50 | 65 |
| knife | L | 20 | 60 | 40 | 70 | 48 |
| dominate | T | 100 | 90 | 50 | 70 | 78 |
| nominate | L | 50 | 60 | 40 | | 50 |
| dove | T | 80 | 90 | 100 | 90 | 90 |
| tough | L | 60 | 60 | 80 | 40 | 60 |
| drew | T | 80 | 40 | 60 | 60 | 60 |
| true | L | 60 | 50 | 30 | 70 | 53 |
| drink | T | 90 | 90 | 70 | 80 | 83 |
| trick | L | 40 | 70 | 20 | 30 | 40 |
| drinker | T | 40 | 40 | 70 | 90 | 60 |
| trigger | L | 80 | 60 | 40 | 40 | 55 |

| Stimuli | | H | B | F | A | Av. for all subjects |
|---|---|---|---|---|---|---|
| dry | T | 70 | 50 | 70 | 50 | 60 |
| try | L | 60 | 70 | 70 | 50 | 60 |
| fake | T | 100 | 100 | 90 | 90 | 95 |
| vague | L | 50 | 30 | 80 | 40 | 50 |
| fawn | T | 100 | 100 | 100 | 100 | 100 |
| fought | L | 60 | 80 | 90 | 60 | 73 |
| felt | T | 100 | 100 | 100 | 100 | 100 |
| felled | L | 30 | 40 | 20 | 50 | 35 |
| fife | T | 100 | 80 | 100 | 100 | 95 |
| five | L | 100 | 100 | 90 | 40 | 83 |
| fin | T | 100 | 100 | 100 | 100 | 100 |
| fit | L | 40 | 50 | 70 | 50 | 53 |
| firm | T | 100 | 90 | 100 | 80 | 93 |
| verb | L | 30 | 40 | 50 | 40 | 40 |
| flick | T | 90 | 100 | 100 | 100 | 98 |
| fling | L | 50 | 50 | 90 | 50 | 60 |
| fret | T | 100 | 100 | 100 | 100 | 100 |
| friend | L | 70 | 50 | 60 | 40 | 55 |
| fix | T | 90 | 40 | 100 | 80 | 78 |
| figs | L | 60 | 40 | 70 | 40 | 53 |
| germ | T | 100 | 90 | 80 | 100 | 93 |
| chirp | L | 70 | 80 | 80 | 50 | 70 |
| gloat | T | 100 | 20 | 10 | 100 | 58 |
| glowed | L | 70 | 60 | 50 | 30 | 53 |
| gnash | T | 100 | 100 | 40 | 60 | 75 |
| dash | L | 70 | 70 | 100 | 30 | 63 |
| grand | T | 100 | 100 | 70 | 90 | 90 |
| grant | L | 40 | 80 | 90 | 80 | 73 |
| grin | T | 100 | 100 | 100 | 100 | 100 |
| grit | L | 50 | 80 | 100 | 80 | 78 |
| halt | T | 100 | 90 | 90 | 100 | 95 |
| hauled | L | 80 | 30 | 90 | 90 | 73 |
| home | T | 100 | 100 | 100 | 100 | 100 |
| hope | L | 70 | 70 | 70 | 70 | 70 |

| Stimuli | | H | B | F | A | Av. for all subjects |
|---|---|---|---|---|---|---|
| hound | T | 100 | 100 | 80 | 100 | 95 |
| out | L | 50 | 40 | 20 | 60 | 43 |
| ice | T | 90 | 100 | 90 | 90 | 93 |
| eyes | L | 70 | 70 | 90 | 50 | 70 |
| lash | T | 100 | 50 | 90 | 80 | 80 |
| latch | L | 40 | 20 | 60 | 60 | 45 |
| lead | T | 60 | 100 | 50 | 90 | 75 |
| lean | L | 10 | 70 | 70 | 90 | 60 |
| loom | T | 100 | 100 | 100 | 100 | 100 |
| loop | L | 30 | 90 | 90 | 60 | 68 |
| lender | T | .. | 90 | 100 | 90 | 93 |
| letter | L | .. | 60 | 70 | 60 | 63 |
| lumber | T | .. | 90 | 100 | 100 | 97 |
| lubber | L | .. | 80 | 100 | 90 | 90 |
| mumps | T | 90 | 80 | 60 | 70 | 75 |
| bumps | L | 20 | 60 | 30 | 40 | 38 |
| manage | T | 100 | 80 | 90 | 100 | 93 |
| bandage | L | 80 | 40 | 40 | 70 | 58 |
| market | T | 100 | 70 | 100 | 90 | 80 |
| bargain | L | 50 | 60 | 60 | 10 | 45 |
| mama | T | 100 | 100 | 80 | 90 | 93 |
| papa | L | 40 | 70 | 50 | 30 | 48 |
| murmur | T | 100 | 100 | 100 | 90 | 98 |
| pepper | L | 70 | 70 | 50 | 60 | 63 |
| music | T | 100 | 100 | 100 | 90 | 98 |
| musing | L | 70 | 70 | 60 | 50 | 63 |
| next | T | 70 | 50 | 60 | 70 | 63 |
| text | L | 60 | 50 | 30 | 30 | 43 |
| noise | T | 60 | 60 | 70 | 50 | 60 |
| toys | L | 90 | 50 | 30 | 50 | 55 |
| pluck | T | 50 | 80 | 30 | 60 | 55 |
| plug | L | 70 | 40 | 40 | 30 | 45 |

| Stimuli | | H | B | F | A | Av. for all subjects |
|---|---|---|---|---|---|---|
| shame | T | 100 | 100 | 100 | 100 | 100 |
| shape | L | 80 | 70 | 100 | 60 | 78 |
| supper | T | 70 | 100 | 100 | 100 | 93 |
| summer | L | 40 | 80 | 50 | 40 | 53 |
| thumb | T | 100 | 100 | 100 | 100 | 100 |
| thump | L | 50 | 60 | 60 | 90 | 65 |
| view | T | 50 | 70 | 60 | 60 | 60 |
| few | L | 60 | 50 | 70 | 60 | 60 |
| amber | T | .. | 80 | 90 | 50 | 73 |
| hammer | L | .. | 30 | 20 | 60 | 37 |
| hamper | | | | | | |
| bled | T | .. | 80 | 20 | 60 | 53 |
| blent | L | .. | 20 | 10 | 10 | 13 |
| blend | | | | | | |
| crumble | T | ... | 40 | 60 | 30 | 43 |
| crumple | L | .. | 20 | 60 | 30 | 37 |
| grumble | | | | | | |
| decrease | T | .. | 90 | 60 | 70 | 73 |
| decrees | L | .. | 10 | 80 | 60 | 50 |
| degrees | | | | | | |
| drub | T | 70 | 40 | 50 | 30 | 48 |
| drum | L | 50 | 60 | 80 | 10 | 50 |
| trump | | | | | | |
| indebted | T | 40 | 40 | 60 | 30 | 43 |
| indented | L | 30 | 30 | 30 | 40 | 33 |
| intended | | | | | | |
| mar | T | .. | 60 | 60 | 40 | 53 |
| bar | L | .. | 30 | 50 | 40 | 40 |
| par | | | | | | |
| muddle | T | .. | 40 | 70 | 100 | 70 |
| puddle | L | .. | 20 | 80 | 10 | 37 |
| bundle | | | | | | |
| snub | T | 20 | 40 | 20 | 60 | 35 |
| stub | L | 60 | 60 | 20 | 20 | 40 |
| stump | | | | | | |

| Stimuli | | H | B | F | A | Av. for all subjects |
|---|---|---|---|---|---|---|
| weed | T | 100 | 90 | 50 | 90 | 83 |
| wean | L | 70 | 60 | 30 | 20 | 45 |
| wheat | | | | | | |
| arbor | | | | | | |
| harper | T | 40 | 40 | 50 | 50 | 45 |
| armor | L | 20 | 10 | 40 | 40 | 28 |
| harbor | | | | | | |
| barge | | | | | | |
| marsh | T | 80 | 60 | 50 | 50 | 60 |
| march | L | 40 | 40 | 10 | 40 | 33 |
| parch | | | | | | |
| birch | | | | | | |
| merge | T | 100 | 50 | 60 | 60 | 68 |
| purge | L | 50 | 20 | 20 | 10 | 25 |
| perch | | | | | | |
| bump | | | | | | |
| mum | T | 100 | 40 | 30 | 40 | 53 |
| pump | L | 70 | 50 | 60 | 40 | 55 |
| pup | | | | | | |
| ground | | | | | | |
| crowd | T | 50 | 30 | 30 | 60 | 43 |
| crown | L | 40 | 60 | 30 | 20 | 38 |
| crowned | | | | | | |
| remain | | | | | | |
| rebate | T | 70 | 50 | 80 | 70 | 68 |
| repaid | L | 60 | 60 | 50 | 50 | 55 |
| remained | | | | | | |
| badge | | | | | | |
| batch | T | 75 | 40 | 40 | 60 | 54 |
| mash | L | 20 | 25 | 20 | 10 | 19 |
| match | | | | | | |
| patch | | | | | | |
| big | | | | | | |
| mink | T | 60 | 15 | 75 | 35 | 46 |
| pick | L | 40 | 15 | 15 | 20 | 23 |
| pig | | | | | | |
| pink | | | | | | |
| bob | | | | | | |
| mob | T | 35 | 25 | 35 | 75 | 43 |
| mop | L | 35 | 5 | 20 | 15 | 19 |
| bomb | | | | | | |
| pop | | | | | | |

| Stimuli | | H | B | F | A | Av. for all subjects |
|---|---|---|---|---|---|---|
| back<br>bag<br>bank<br>bang<br>pang | T<br>L | 65<br>5 | 25<br>20 | 35<br>30 | 80<br>20 | 51<br>19 |
| crab<br>cram<br>cramp<br>grab<br>gram | T<br>L | 40<br>25 | 45<br>10 | 20<br>15 | 35<br>5 | 38<br>14 |
| duck<br>dug<br>tongue<br>tuck<br>tug | T<br>L | 80<br>30 | 50<br>20 | 30<br>40 | 30<br>15 | 48<br>26 |
| banner<br>banter<br>manner<br>batter<br>matter<br>patter | T<br>L | 50<br>10 | 40<br>15 | 45<br>30 | 15<br>15 | 38<br>18 |
| blade<br>plain<br>plate<br>plaint<br>planed<br>played | T<br>L | 35<br>35 | 25<br>5 | 50<br>40 | 35<br>5 | 36<br>21 |
| drain<br>trade<br>drained<br>train<br>trained<br>trait | T<br>L | 55<br>35 | 60<br>30 | 55<br>50 | 55<br>20 | 56<br>34 |
| fan<br>fad<br>van<br>fat<br>vat<br>fanned | T<br>L | 30<br>45 | 35<br>15 | 60<br>20 | 65<br>5 | 48<br>21 |
| munch<br>much<br>mush<br>budge<br>bunch<br>punch | T<br>L | 25<br>20 | 30<br>20 | 20<br>30 | 35<br>20 | 28<br>23 |

| Stimuli | | H | B | F | A | Av. for all subjects |
|---|---|---|---|---|---|---|
| bound<br>bout<br>mound<br>pound<br>bowed<br>mount<br>pout | T<br>L | 15<br>25 | 25<br>30 | 35<br>30 | 30<br>15 | 26<br>25 |
| brick<br>brig<br>brink<br>prig<br>bring<br>prick<br>prink | T<br>L | 25<br>10 | 25<br>20 | 20<br>20 | 40<br>15 | 28<br>16 |
| mince<br>pins<br>bins<br>pits<br>bits<br>mitts<br>bids | T<br>L | 15<br>20 | 25<br>15 | 20<br>15 | 55<br>5 | 29<br>14 |
| wed<br>went<br>wen<br>wend<br>wet<br>when<br>whet | T<br>L | 15<br>35 | 30<br>25 | 45<br>5 | 25<br>5 | 29<br>18 |
| dead<br>debt<br>dent<br>tend<br>den<br>ten<br>tent<br>net | T<br>L | 35<br>20 | 30<br>5 | 40<br>25 | 10<br>20 | 29<br>18 |
| buck<br>bug<br>bunk<br>muck<br>pug<br>bung<br>monk<br>mug<br>punk | T<br>L | 40<br>25 | 10<br>10 | 5<br>30 | 10<br>5 | 16<br>18 |

| Stimuli | | H | B | F | A | Av. for all subjects |
|---|---|---|---|---|---|---|
| bend | | | | | | |
| bent | | | | | | |
| mend | | | | | | |
| meant | | | | | | |
| penned | T | 50 | 15 | 15 | 5 | 21 |
| pent | L | 15 | 10 | 30 | 20 | 19 |
| bed | | | | | | |
| pen | | | | | | |
| bet | | | | | | |
| pet | | | | | | |

groups in which the subjects as a body did not make a better score by touch than by vision. In two of the eight cases, the score was equal by vision and touch. This, of course, is a *long* way from saying that touch may be reasonably expected to substitute for vision in the reception of speech. It means that, in relation *to these particular words,* touch furnishes more clues than does vision. By this token, one may justifiably assume, without further trial, that by feeling speech and seeing it simultaneously one will have an advantage over only seeing it. As far as lip-reading goes, the word *mat* in a sentence might be *bat,* but the tactual cue makes it certainly *mat* (assuming that these alternatives have occurred to the subject) and thus saves the meaning of the sentence. (In very many cases, of course, *context* would bring the true meaning without the addition of any sensory cue.)

Beginning with the first of our groups of two each in Table 1, we have selected the first ten pairs in respect to which the subjects' average difference between L and T scores was no less than 35. In like manner, we have chosen the first ten pairs that show an average difference for the group of no more than 13 (we have excluded exceptions to the general rule that the T score is higher than the L score).

The pairs selected are given in Table 2.

Comparison of pairs in the two columns of Table 2 reveals that in the second column there are six pairs, the members of which have the same ending (consonant or vowel). There are five that have the same initial consonant or consonants. As the subjects say, they have the same "clicks" (the same degrees of *sharpness of punch*).

In the first column are only two that have approximately the same

TABLE 2
SOME OF THE CHARACTERISTICS OF WORDS BY WHICH SUBJECTS ARE ABLE TO MAKE A DIFFERENCE BETWEEN T AND L

| | | Av. diff. between L & T | | | Av. diff. between L & T |
|---|---|---|---|---|---|
| 1. | lash–latch | 35 | 11. | best-pest | 2 |
| 2. | fret–friend | 37 | 12. | gloat-glowed | 5 |
| 3. | flick–fling | 38 | 13. | noise-toys | 5 |
| 4. | supper–summer | 40 | 14. | lumber-lubber | 7 |
| 5. | drink–trick | 43 | 15. | drew-true | 7 |
| 6. | fake–vague | 45 | 16. | pluck-plug | 10 |
| 7. | mama–papa | 45 | 17. | chair-share | 10 |
| 8. | fin–fit | 47 | 18. | gnash-dash | 12 |
| 9. | firm–verb | 53 | 19. | fife-five | 12 |
| 10. | felt–felled | 65 | 20. | cream-creep | 13 |

endings and five with approximately the same beginnings. The differences of consonantal composition in the first column are those that have been easily distinguished tactually in other connections in our laboratory. Corresponding differences in the second column are less easily distinguished. (See *J. Franklin Instit.*, 1927, **204**, 334-336.)

It is apparent that the initial or final (or both initial and final) elements of the words are largely responsible for the superiority. The Laboratory, however, has repeatedly shown that subjects can learn to distinguish and identify vowel and diphthongal qualities with a high degree of accuracy. (See, e. g., *J. Franklin Instit.*, 1927, **204**, 346ff.)

### INDIVIDUAL SCORES

Computations based upon all the scores of individual subjects by T and by L, respectively, will be more to the point than the average performances of the group.

TABLE 3
EXCEPTIONS TO THE GENERAL RULE THAT T IS SUPERIOR TO L

| | 1 L superior to T—no. of groups | 2 Total no. of groups | 3 L equals T—no. of groups | 4 Percentage of exceptions |
|---|---|---|---|---|
| H | 11 | 94 | 3 | 14.4 |
| B | 14 | 99 | 6 | 20.2 |
| F | 15 | 94 | 13 | 29.7 |
| A | 9 | 102 | 9 | 17.6 |

According to the record, subjects usually make a higher score by T than by L. There are exceptions as given in Table 3.

Comparing the figures in columns 1 and 3 of Table 3, we find 80 exceptions (out of 389 cases—20.6%) to the general rule that in relation to these homophenous words our subjects make a higher record by T than by L.

TABLE 4

L Records Equal or Superior to the T Records of Two or More Subjects in the Following Groups

| | Subjects |
|---|---|
| brag–prank | H, B, F |
| cream–creep | H, F |
| chair–share | B, A |
| chafe–shave | H, B |
| drew–true | B, A |
| drinker–trigger | H, B |
| dry–try | B, F, A |
| fife–five | H, B |
| gloat–glowed | B, F |
| lead–lean | F, A |
| noise–toys | H, A |
| pluck–plug | H, F |
| view–few | H, F, A |
| crumple–crumble–grumble | F, A |
| drub–drum–trump | B, F |
| snub–stub–stump | H, B, F |
| bump–mum–pump–pup | B, F, A |
| ground–crowd–crown–crowned | B, F |
| bound–bout–mound–pound–bowed–mount–pout | H. B |
| buck–bug–bunk–muck–pug–bung–monk–mug–punk | B, F |
| bend–bent–mend–meant–penned–pent–bed–pen–bet–pet | F, A |

No. of groups, 21. Total exceptions to the general rule by two or more subjects, 46.

The 21 groups in Table 4, out of the total of 102, furnish 46 of the total of 80 exceptions to the general rule. In other words, approximately one-fifth of the total number of groups are responsible for more than one-half of the exceptions.

In the Tables 5-11, we show how each subject's T record compares with his L record when all the groups of two and three, etc., are taken together in a mass.

TABLE 5
AVERAGE PERCENTAGE PER GROUP OF TWO
(Chance result would be approximately 50%)

| | T | L |
|---|---|---|
| H | 86.3 | 55.4 |
| B | 82.2 | 53.7 |
| F | 83.1 | 61.9 |
| A | 83.1 | 51.4 |

TABLE 6
AVERAGE PERCENTAGE PER GROUP OF THREE
(Chance result would be approximately 33%)

| | T | L |
|---|---|---|
| H | 57.5 | 52.5 |
| B | 60.0 | 34.0 |
| F | 54.0 | 46.0 |
| A | 56.0 | 30.0 |

TABLE 7
AVERAGE PERCENTAGE PER GROUP OF FOUR
(Chance result would be approximately 25%)

| | T | L |
|---|---|---|
| H | 73.3 | 46.6 |
| B | 45.0 | 40.0 |
| F | 48.3 | 35.0 |
| A | 55.0 | 33.3 |

TABLE 8
AVERAGE PERCENTAGE PER GROUP OF FIVE
(Chance result would be approximately 20%)

| | T | L |
|---|---|---|
| H | 59.1 | 25.8 |
| B | 33.3 | 15.8 |
| F | 40.8 | 23.3 |
| A | 52.5 | 14.1 |

TABLE 9
AVERAGE PERCENTAGE PER GROUP OF SIX
(Chance result would be approximately 16%)

| | T | L |
|---|---|---|
| H | 39.0 | 33.0 |
| B | 38.0 | 17.0 |
| F | 46.0 | 34.0 |
| A | 41.0 | 13.0 |

TABLE 10
AVERAGE PERCENTAGE PER GROUP OF SEVEN
(Chance result would be approximately 14%)

| | T | L |
|---|---|---|
| H | 17.5 | 22.5 |
| B | 26.2 | 22.5 |
| F | 30.0 | 17.5 |
| A | 37.5 | 10.0 |

TABLE 11
AVERAGE PERCENTAGE PER GROUP OF EIGHT, NINE, AND TEN, RESPECTIVELY*
(Chance results would be approximately 12%, 11%, and 10%, respectively)

| Group of | H | | B | | F | | A | |
|---|---|---|---|---|---|---|---|---|
| | T | L | T | L | T | L | T | L |
| 8 | 35 | 20 | 30 | 5 | 40 | 25 | 10 | 20 |
| 9 | 40 | 25 | 10 | 10 | 5 | 30 | 10 | 5 |
| 10 | 50 | 15 | 15 | 10 | 15 | 30 | 5 | 20 |

*There was but one group of eight, nine, and ten, respectively.

## SUMMARY

Homophenous words, to the number of 305, were taken for study. They fall into 102 groups of two, three, etc.

After a definite amount of preliminary drill, how accurately can four deaf subjects discriminate the members of a group by lip-reading in 40 trials? Similarly, in 40 trials, how accurately can they discriminate the same words *by touch alone* when the speaker is out of sight in an adjoining room?

In only eight groups did the subjects as a whole fail to make a better record by tactual cues alone than by visual cues alone.

Taking the records of subjects individually, out of 389 cases, there are only 80 exceptions to the general rule that, *in re* our homophenous words, our subjects make a better record by touch than by vision. One-fifth of the total number of groups, approximately, is responsible for approximately one-half the exceptions.

## PROSPECTIVE TESTS AND CONCLUSION

We plan to repeat this test upon other subjects and to use only word groups of uniform size. We have already repeatedly made similar tests employing *non-homophenous* words in connection with our work on interpretation of speech when the feel of speech supplements vision of the speaking face. The addition of the tactual cue in those cases increased the accuracy of interpretation.

The general conclusion from this work is that, on the whole,

homophenous words in small groups of two to ten can be discriminated by touch more accurately than by vision of the speaker's face.

This is one point at which the combination of the feel and the vision of speech helps the deaf interpreter of spoken language. At the same time, there are doubtless groups in the grand total of 2,000 homophenous words, in addition to those we have already found, the members of which cannot be discriminated by touch.

Incidentaly, this study by inference opposes the hypothesis that subjects who interpret speech more successfully when tactual and visual cues are combined than when visual signs stand alone do so merely because of *accessory stimulation*—as if holding a continously actuated buzzer in the hand while the experimenter is verbally presenting his stimuli would result in improved reception. In other circumstances, this hypothesis might be worthy of attention. But in view of the data in this paper and in another paper on the "Interpretation of Spoken Language when the Feel of Speech Supplements Vision of the Speaking Face,"[2] the hypothesis referred to is certainly inferior to this one: that tactual stimuli arising from speech that is visible to the subjects aid interpretation.

This work has an additional important bearing upon our study of the interpretation of speech via combined tactual and visual stimuli. Someone may ask the question whether tactual stimuli from spoken language, simultaneous with visual stimuli from the same source, do not afford only an indirect advantage merely as distraction. It is a very far-fetched question in view of the data we have presented under the title: "Interpretation of Spoken Language," etc. In this work on homophenous words, there is no simultaneity of visual and tactual cues. The tactual experience evidently brings forward cues that contribute something positive *beyond what vision offers as media for recognition.*

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[2]Publication arranged for.

## LA DISTINCTION DES MOTS HOMOPHÈNES PAR DES SIGNES TACTILES

(Résumé)

Les mots "homophènes" se ressemblent pour celui qui lit les lèvres. "Grand" et "grant" sont des exemples. Il y a environ 2000 tels mots dans la langue anglaise. Ils se trouvent en groupes de deux, de trois, ou plus. Cette étude tâche de savoir l'exactitude avec laquelle on peut identifier de telles formes isolées, au moyen du toucher et de la lecture des lèvres, respectivement, après un minimum d'exercice—cinq lectures par l'expérimentateur dans un ordre connu. Les mots servant de stimuli se trouvent en 102 groupes, et font un total de 305.

En général, le stimulus tactile seul permet une meilleure distinction que le stimulus visuel seul (lecture des lèvres). Les éléments initials ou finals (ou initials et finals) sont pour la plupart responsable de la supériorité. Le Laboratoire, pourtant, a montré très souvent que les sujets peuvent apprendre à distinguer et à identifier les qualités des voyelles et des diphtongues avec une très grande exactitude.

Les résultats des quatre sujets individuellement ne montrent que 80 exceptions sur 389 cas (20.6%) à la règle générale citée en haut. Approximativement un cinquième des 102 groupes est responsable de 46 exceptions—plus que la moitié. Ces chiffres signifient que le toucher peut contribuer des signes de significations dans la langue parlée lesquels échappent à celui qui lit les lèvres. Le Laboratoire a fait 77 démonstrations (84 essais) d'une interprétation supérieure (avantage médian 30%) quand des sujets sourds simultanément ont le sens des mots et voient la figure de celui qui parle.

GAULT

## DIE UNTERSCHEIDUNG GLEICHLAUTENDER WÖRTER DURCH FÜHLBARE ZEICHEN

(Referat)

"Gleichlautende Wörter" erscheinen dem Lippenleser gleich; *grand* und *grant* zum Beispiel. Es giebt über 2000 solcher Wörter in der englischen Sprache. Man findet sie in Gruppen von zwei, drei oder mehr Wörtern. Der Zweck dieser Studie ist nun zu ermitteln wie genau solche Formen einzeln identifiziert werden können, entweder durch Betasten oder Lippenlesen und zwar nach einem Minimum der Übung—fünfmaliges Lesen des Versuchsleiters bei einer gewissen Anordnung. Die Stimuluswörter zerfallen in 102 Gruppen, im ganzen 305 Wörter.

Man findet im allgemeinen, dass der fühlbare Stimulus allein ein feineres Unterscheidungsvermögen erfordert als der sichtbare Stimulus (Lippenlesen) allein. Die ersten oder letzten (oder die ersten und die letzten) Elemente bedingen hauptsächlich die höhern Resultate. Im Laboratorium fand man jedoch dass Vpn. lernen können, die vokalen und diphtongischen Merkmale mit grosser Genauigkeit zu unterscheiden und zu identifizieren.

Der Rekord der vier Vpn. einzeln genommen zeigte nur 80 Ausnahmen in 389 Fällen (20.6%) von der oben erwähnten allgemeinen Regel. Ungefähr ein fünftel der 102 Gruppen erwiesen 46 Ausnahmen—über die Hälfte. Diese Zahlen zeigen dass der Tastsinn Zeichen der Bedeutung der gesprochenen Sprache wahrnehmen kann, die dem Lippenleser entgehen. Man hat im Laboratorium 77 Demonstrationen (84 Versuche) höherer Interpretation ausgeführt (mittlerer Vorteil 30%), wenn Taubstumme gleichzeitig die Sprache "fühlen" und das Gesicht des Sprechenden sehen.

GAULT

## СПОСОБНОСТЬ РАЗЛИЧАТЬ ОДИНАКОВО ЗВУЧАЩИЕ СЛОВА ПО ТАКТИЛЬНЫМ ПРИЗНАКАМ.

(Реферат).

Одинаково звучащие слова кажутся совершенно идентичными, если воспроизводить их мимическим движением губ. „Grand" и „Grant" могут служить примерами такого сходства В английском языке имеется более 2.000 подобных слов Они встречаются группами в два, три и больше слов Настоящее исследование имело целью установить, в какой степени такие изолированные формы могут быть отождествлены прикосновением и мимическим движением губ, после нескольких упражнений—чтение экспериментатором в течение пяти раз в известном порядке Слова-раздражители разделяются на 102 группы; всех слов было 305.

В общем, тактильные раздражители дают лучшую способность к различению, чем только мимическое движение губ Первые или последние (или те и другие вместе) элементы влияют на результаты. Опыты в лаборатории указывают, однако, что испытуемые в состоянии научиться различать и отождествлять гласные и двугласные довольно точно

Индивидуальные результаты четырех испытуемых дают только 80 исключений к общему правилу из 389 случаев (20,6%). Приблизительно одна пятая всех 102 групп дает 46 исключений. Эти цифры означают, что осязание может установить такие признаки значения в разговорном языке, которые незаметны при мимическом движении губ. Лаборатория провела 77 демонстраций (84 попытки), получив лучшие результаты (на 30%), когда глухой испытуемый одновременно воспринимает и видит лицо говорящего.

**Голт (Gault).**

# THE BEARING OF PROFESSOR PAVLOV'S WORK ON THE PROBLEM OF INHIBITION*

*From the Department of Psychology of Duke University*

WILLIAM MCDOUGALL

The publication of the English translation of Professor Pavlov's lectures[1] makes possible for the first time to English and American readers full appreciation of the immensely important results obtained in the Petrograd laboratories by the intensive study of the conditioned reflex ardently sustained through twenty years by the Professor and his numerous collaborators. The history of science presents, I believe, no parallel to this concentrated sustained cooperative attack on one great problem by one brilliantly developed new method. The results attained are of extreme interest from many points of view. In this article I am chiefly concerned with their bearing on the problem of the nature of the inhibitory process in the nervous system. Fully half of the pages of this large book are concerned with inhibitory phenomena. One turns, therefore, with eager curiosity to find what view Professor Pavlov has reached as to the intimate nature of the inhibitory process, but only to meet with disappointment.

Pavlov abstains from all attempt to formulate an hypothesis as to the nature of the inhibitory process. "In this research we were not concerned with the ultimate nature of excitation and inhibition as such. We took them as two fundamental properties, the two most important manifestations of activity, of the living nervous elements. Nor was it our aim to interpret the activity of the hemispheres in terms of elementary functions of the nervous system . . . .We intentionally neglected also the controversial problem of the actual localization of these two fundamental processes, and did not attempt to assign them to either of the two elements of the nervous structure, namely, the nerve cell and the synaptic junction or fibrillary connection between two individual nerve cells. The ac-

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[1]Pavlov, I. P. Conditioned reflexes: an investigation of the physiological activity of the cerebral cortex. (Trans. by G. V. Anrep.) London: Oxford Univ. Press, 1927.

ceptance of the more general conception of the two processes of inhibition and excitation as the basic functions of the nervous cellular structures was sufficient for the purposes of our research" (p. 377).

Nevertheless, since "fools rush in where angels fear to tread," I permit myself to point out in this article that all of the many phenomena of inhibition so brilliantly studied and reported in this great work seem to lend strong support to the hypothesis of inhibition by drainage which I formulated twenty-five years ago and applied to the explanation of a variety of phenomena occurring in all levels of the nervous system, especially the reciprocal inhibitions of the spinal level, the reciprocal inhibitions of instinctive processes in the basal ganglia, the inhibitions on the sensory level, especially the phenomena of visual contrast and, above all, of graded contrast, and the reciprocal inhibitions of the higher levels manifested in the focal nature of "attention," the great fact of concentration of psychophysical energy along one system of channels demonstrated in all bodily and mental effort.[2]

When Sir Charles Sherrington's great book on *The Integrative Action of the Nervous System* was published, I ventured to point out that my hypothesis, although Sherrington examined and rejected it, was compatible with all the phenomena of inhibition described by him and remained the only hypothesis that seemed remotely capable of interpreting the facts. I venture now to repeat this claim in relation to the great new wealth of inhibitory phenomena of the cerebral

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[2]McDougall, W. The nature of inhibitory processes in the central nrevous system. *Brain,* 1903, **26,** 153-191.

McDougall, W. Primer of physiological psychology. New York: Macmillan, 1905. Pp. xiii+172.

The hypothesis of inhibition by drainage was further developed and applied to the interpretation of cerebral events in my little-known series of articles in *Mind* (1902-1906, **11-15,** N. S.) entitled, "The Physiological Factors of the Attention Process." The evidence from visual contrast phenomena was adduced in two papers in *Proceedings fo the Physiological Society* and the *Journal of Physiology,* March and July, 1903. In the article in *Mind* I stated the hypothesis concisely as follows: "The excitement of any sensori-motor arc diminishes the resistance that it offers to the onward passage towards its efferent neurones of the current of free nervous energy or neurin, and its resistance is the more diminished the greater the intensity of its excitement. If then two or more such arcs are connected together in their central parts, that one which is the most intensely excited will become for the time being the path of lowest resistance for the escape of neurin to the motor neurones, and will, therefore, tend to drain to itself and to discharge by way of its motor neurones the energy liberated in all the others."

hemispheres described by Pavlov. And I may go farther and point out that, though Pavlov refuses, as we have seen, to indulge in any speculation as to the nature of the inhibitory processes he describes, yet his language on many occasions seems to imply and almost consciously to assume the truth of the hypothesis of inhibition by drainage.

Before proceeding to examine the bearing of the new evidence, it may be worth while to restate the hypothesis, its implications, and the principal objections to it which have prevented it from finding favor with physiologists and psychologists.

If I go to my bathroom, where the water supply is delivered under very considerable pressure, and turn on the small faucet in the wash-basin until a small steady stream of water issues from it, and if then I turn on the larger faucet in the bath-tub, the smaller stream of water from the former diminishes in volume as the second stream gathers volume; the former is completely or partially inhibited according to the relative volumes of the two streams. That is one form of inhibition by drainage. A closer analogy perhaps is provided by the following arrangement. Two parallel pipes carrying water under a certain pressure are connected by an open cross pipe; their outlets are controlled by faucets. If the one faucet is more widely open than the other, allowing a more rapid flow in its pipe, the fuller, more rapid stream will draw to itself water from the other stream and, by so doing, diminish the volume of the flow from the other faucet. I am told by physicists that a similar drainage would result if the arrangement consisted of wires carrying electric currents. Now place in each circuit at intervals a number of Nernst lamps, or any glowlamps of carbon filaments (i. e., resistances whose resistance diminishes in proportion to the volume of the current). These would correspond to the synapses on the nerve paths, for, as I have pointed out, it is in accordance with the evidence to suppose that the resistance of the synapse is lowered on excitation of the neurones between which it lies (until synaptic fatigue sets in and again raises the resistance). We then have an arrangement still more favorable to the production of reciprocal drainage, drainage from the weaker to the stronger current. The synapses will act like spring-controlled faucets which open the more widely, the stronger the pressure of the water upon them.

It is of the essence of this view that inhibition in the central

nervous system is always a complementary or collateral effect in one neurone system of a process of excitation in another neurone system; and it is implied that the excitation process of the inhibiting system gains in intensity (or volume of energy transmitted through its neurones) at the expense of the inhibited system.[3]

Two difficulties chiefly stand in the way of the acceptance of this view. First, it implies the assumption that in the spread of nervous excitation energy of some sort liberated in the excited neurones (presumably by some katabolic process) flows along the neurones, passes across synapses into other neurones, and takes part in the processes of excitation and conduction in them. In short, it assumes that the neurone plays a part analogous both to an electric battery and to the wires of the electric circuit into which the generated current flows. This view, which in my early papers I designated as the theory of vicarious usage of nerve energy, has always been rejected by the great majority of physiologists, chiefly, I think, on the insufficient ground that we have no direct and indisputable evidence of such flow of nerve energy from neurone to neurone. That such transmission and discharge from the tissue that is the seat of liberation of energy is possible cannot be denied. The phenomena of the electric fishes, torpedo, and others seem to be indisputable instances of such discharge. Further, almost all writers who have discussed the functions of the nervous system from the point of view of students of the whole organism and its functions, rather than

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[3] I cite again from my articles in *Mind:* "If, in accordance with the evidence adduced in an earlier article of this series, we regard the neural system of the upper brain levels as so many organized channels by which a common store of free energy, contained in the mass of afferent neurones and maintained in them at a varying pressure or potential by the streams of energy flowing in from the sense organs [I should now add—and from the instinctive centers in the basal ganglia] is discharged in efferent channels; and if also we assume, as we have seen good reason to do, that during the excitement of any such channel its normal or resting resistance is lowered in proportion to the intensity of its excitement, then it follows that the excitement of any one such system must tend to inhibit that of every other, and that the system which at any moment is rendered, by a constellation of favoring conditions, the path of lowest resistence from afferent to efferent side of the brain will divert to itself and transmit to its efferent neurons the energy which is liberated in the afferent neurons of any other system simultaneously excited and so will inhibit that system" (1906, **15**, 352). Dr. L. F. Richardson F.R.S. allows me to refer to the fact that in an article now in the press he develops this electrical analogy and describes an arrangement (in which neon lamps take the place of synapses) which illustrates the drainage process as here conceived.

that of students of the functions of isolated tissues, have found themselves led to make use of some such conception. Since I pointed to this fact nearly thirty years ago, the fact has gained weight or evidence through the development of several schools of psychology.[4] The "vicarious usage" view is implied in all the writings of the various psychoanalytic schools; and, however we may regard their views, we cannot refuse to recognize that they have contributed greatly to the progress of psychology and psychiatry. It is, it seems to me, though here I write subject to correction, implied in the views of the writers of the now so influential school of Gestalt; e. g., Professor Köhler writes of unitary fields of energy in the brain, within which fields every new sensory stimulus effects some redistribution of energy; and it seems to be even more clearly implied by the very interesting work on conation of another member of the same school, namely, Dr. Kurt Lewin.[5] The same view is implicit in Professor Spearman's discussion of mental energy[6] and in the view (which he inclines to) that his *g* factor represents the quantity of disposable nervous or mental energy.

Professor E. Rignano has developed in a numbor of books and articles[7] his interesting views on the all-important affective tendencies and his centro-epigenetic hypothesis, all of which postulate the transmission and vicarious usage of nervous energy.

The same view is implied by the experiments by Dr. K. S. Lashley reported at the meeting of the American Psychological Association in December, 1927. Dr. Lashley has extirpated in many rats various portions of the cerebral hemispheres and has found that their power of "learning" was in general diminished in proportion to the amount of cerebral substance removed, from no matter what region. He thus confirms conclusions drawn long ago by Golz, Pitres, and Féré

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[4]In a recent review of work on facilitation and inhibition in the *Psychological Bulletin,* the author summarily dismisses my hypothesis on the sole ground that "its dependence on an exploded concept of 'neurin' makes it untenable." I am not acquainted with any facts or arguments that may have effected this alleged explosion. My aim here is to show that the "concept" is increasingly demanded and accepted.

[5]Lewin, K. Vorsatz Wille und Bedürfnis mit Vorbemerkungen über die Psychischen Kräfte und Energien und die Strukter der Seele. Brelin: Springer, 1926. Pp. 92.

[6]Spearman, C. E. Abilities of man. London and New York: Macmillan, 1927. Pp. xxiii+415.

[7]Especially in his lecture before the College de France in the year 1920, entitled "The Purposefulness of Life."

(the last wrote "the destruction of any cerebral centre whatever produces in general a certain degree of intellectual enfeeblement"). It is perhaps permissible to reveal the fact that in conversation Dr. Lashley agreed that his results seemed to point to the vicarious usage of nervous energy. Lastly, it is explicitly accepted in the dynamic or hormic psychology which I have long striven to develop in many books and articles and which, though it can claim but few exponents in America, is very generally accepted by British psychologists. It is a principal aim of this article to show that the same view is implied by the observations and deductions of Professor Pavlov and his fellow-workers.

If, then, students of the organism as a whole of so many schools agree in finding need of the "vicarious usage" view, the fact goes far to establish it as a necessary working hypothesis which cannot and should not be ruled out of court because those physiologists whose work consists in the study of fragments of living tissue have failed to find direct evidence in support of it or to feel the need of it.

Thirdly, I insist once more on the much neglected but, to my mind, highly significant phenomenon of the march or spread of excitation that accompanies the onset of local fatigue in ergographic experiments. This phenomenon seems to imply very clearly the overflow of concentrated nerve energy into collateral channels owing to rising synaptic resistances in the primary channel of discharge.

Fourthly, I insist once more on the neglected but profoundly important fact that in muscular as in mental effort we do seem effectively to concentrate energy along one system of neurones, with complementary inhibition of other nervous and mental activities and that in animals we observe indications of similar concentration (with complementary inhibitions) whenever in them an instinctive mode of activity is strongly excited, a fact which Pavlov's work amply illustrates.

The second main difficulty in the way of the hypothesis of vicarious usage, and, therefore, also of inhibition by drainage, is the alleged general validity of the all-or-none principle in nerve excitation and conduction. The evidence on which this principle is founded (adduced since the formulation of the drainage hypothesis) has seemed and still seems a formidable objection, for I cannot see that the two views can be reconciled. I am not competent to

discuss the evidence for the all-or-none principle; I can only submit that the immense number of instances of finely graded intensities of nervous process indicated by the phenomena of the fields of sensation, of muscular innervation, and of emotional response seem to me to render the all-or-none principle inacceptable save under the compulsion of the clearest and most indisputable evidence. Here again Pavlov's work strengthens this position by revealing cerebral inhibition as anything but conformable to the all-or-none principle, rather as exhibiting all gradations from slight and partial to complete.

We turn now to point out how Pavlov's observations and conclusions lend support to "inhibition by drainage."

Pavlov distinguishes three main types of cerebral inhibition. The three types of inhibition distinguished are: (1) direct or internal, (2) indirect or external, and (3) sleep. By direct or internal inhibition Pavlov seems to mean the inhibition of an excitatory process in a neurone group by the direct impingement upon it of an impulse or excitation process conducted along some connected nerve path; in explanation of his meaning he says only: "we are familiar with the direct inhibitory action of certain afferent nerves, or of certain physical and chemical properties of the blood, upon definite nervous centres controlling respiration, circulation, locomotion and so on." There is a certain confusion here I think. Surely we risk confusion if we speak of the influence of chloroform on the brain as inhibition! But the conception of cerebral inhibition commonly accepted seems to be modeled upon such instances as that of the vagus nerve on the heart; that is to say, the physiologists commonly think of inhibition within the nervous system as of the type of what Pavlov calls direct or internal inhibition.

Indirect or external inhibition is described as follows: "On the other hand the central nervous system supplies numerous cases of indirect inhibition. In the latter cases inhibition of the activity of a given centre is brought about as the result of an activity of some other centre which activity has arisen in its turn as a result of excitation from some afferent nerve or from some change in the composition of the blood" (p. 44).

Toward the end of the book Pavlov raises the question whether direct and indirect inhibition are not essentially similar. On p. 387 he writes, "Hence a fundamental identity of all the three types of inhibition appeared very probable." He then goes on to adduce

further evidence in support of the view that the so-called instances of direct or internal inhibition are in reality of the same nature as indirect or external inhibition. He has already shown reason to assimilate sleep to internal inhibitions. It follows that all three kinds of inhibition are in all probability really of one type, external or indirect inhibition, i. e., inhibition of excitation in one area or system of neurones in consequence of the stronger excitation of some other neurone system. This conclusion, which Pavlov puts forward as "a strong probability," conforms exactly to the drainage hypothesis.

The drainage hypothesis regards the formation of the new path of low resistance (which is the essential process in the setting-up of the conditioned reflex) as the consequence of the drainage of the one system A by the other B, of the inhibition of the excitation A (initiated by the neutral or conditioned stimulus) by the stronger excitation B (initiated by the conditioning or unconditioned stimulus). It assumes that the unconditioned stimulus sets up an excitation process B, issuing in an efferent discharge which, by offering a path of momentarily lowered resistance, attracts or drains to itself the energy liberated in the other system A by the conditioned stimulus; and it assumes that the passage of the drainage current from A to B results in an enduringly lowered resistance of the path A-B; and that, with repetition of the two stimuli in the same order (i. e., repetition of drainage of A by B) the drainage path A-B becomes so well trodden, so lowered in resistance, that, after a sufficient number of repetitions, a stimulation of A results in the discharge of its excitation through A-B, in the absence of direct stimulation of B.

In formulating the drainage hypothesis, I advanced as the strongest ground in its support the fact that the excitation of any one cerebral system (if it is sufficiently intense) can inhibit any other cerebral system already in a state of excitation; and I urged that only in this way can we interpret the general fact of tthe formation of association paths in the brain. I formulated the law of the attraction of the impulse from the less strongly functioning system to the more strongly functioning system as the general expression of this tendency of the stronger system of excitation to drain the weaker system.[8]

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[8]In my articles in *Mind* I wrote: "Since it must be admitted that the excitement of *any* higher level system B, following immediately upon the excitement of *any other* such system A, may cause the transmission of the impulse or flow of energy from A to B (in the light of the general fact of formation of associations by repeated succession of two cerebral pro-

Pavlov's studies of the conditioned reflexes reveal these processes of interaction between cerebral systems in much greater detail than all previous observations; and the conception of drainage enables us to interpret intelligibly a large proportion of all the phenomena he describes.

Let us notice at once two general facts which are very difficult to reconcile with any view of inhibition other than the drainage view. "One and the same point of the cortex could, in accordance with the given experimental conditions, be a point of origin either of excitation or of inhibition. These transformations of the physiological significance of a definite cortical point are attained with comparative ease and effected with considerable rapidity both in respect of positive and negative stimuli" (p. 228). Secondly, "the intensity of the primary inhibition is found to be exactly proportional to the intensity of the excitatory process on which it is based." The former statement means that the same stimulus applied to the same sensory point may, on one occasion, result in the inhibition of some process under observation, on another in the excitation of some process. Both statements, then, seem to imply that excitation is always the primary result of the stimulus, that in no case does the stimulus initiate a specifically inhibitory process, but rather always an excitatory process of which inhibition is a further and collateral consequence.

Sometimes Pavlov's own language almost seems to imply the drainage view. Thus he describes the formation of the conditioned reflex in the following general terms: "the fundamental mechanism of development of a conditioned reflex depends upon excitation of some definite point in the cortex coincidentally with a more intense

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cesses), it is clear that B must in some way guide the discharge of the impulse from A, must attract to itself the energy liberated in A, so determining it to take just one path, the path A-B, of all the thousand-fold paths open to it. The generalized statement of this conclusion I have elsewhere proposed to call the *law of the attraction of the impulse,* and I have pointed out that it is necessarily implied in the wider law of the formation of neural habits." This is the process which, conceived to account for the events of the cerebrum as reflected in human experience, is now abundantly illustrated by Pavlov's observations on the conditioned cerebral reflex as demonstrated objectively in dogs.

It is a remarkable fact that, while the formation of cerebral associations has continued to be confidently accepted by so many psychologists and physiologists as the sole method of mental growth or acquisition of new adaptations, no one has accepted my hypothesis as to their mode of formation, and during the quarter-century that it has been before the public no one has proposed any intelligible alternative.

excitation of some other point, probably also of the cortex, which leads to a connection being formed between these two points; and reversely, if such a coincident stimulation of these points is not repeated for a long time the path becomes obliterated and the connection disrupted. But when once such a path has been firmly established it remains intact without further practice, for months and years. It is obvious that under suitable conditions a new connection must be formed at the very first occurrence of the simultaneous excitation and become strengthened by every repetition. In ordinary experiments with normal dogs about twenty reinforced repetitions are required to establish the first of any conditioned reflexes experimentally produced." (p. 385).

Now the connection established between A and B (continuing to speak of the system excited by the neutral or conditioned stimulus as A, and that excited by the conditioning or unconditioned stimulus as B) by the repetition in close succession of the two stimuli, A and B, is conclusive evidence that on each occasion some current of energy, some excitation or "nerve impulse" was transmitted along the path A-B; for such transmission along a nerve path is the only conceivable way in which it can be established, can be rendered an open path, a path of low resistance. The question, then, is—in which direction along A-B is the current transmitted on each occasion of the succession of the two stimuli, A and B? Does it go from B to A, or from A to B? This is the crucial question. If we find good reason to believe that the direction is from A to B, we can hardly refuse to accept the drainage hypothesis.

Now Pavlov shows conclusively that the effect of the twenty repetitions of the two stimuli, A and B, in that order is to establish a path open from A to B, for after these repetitions the excitation of A discharges spontaneously into and through B, without prior stimulation of B. But there is no evidence that excitation of B tends to discharge into A, as should be the case if the path A-B is rendered open in both directions. Further, it is a common and well-grounded assumption that the passage of the nerve impulse along any cerebral path tetnds to leave that path more open, of lowered resistance, in that direction (the direction of the transmission), but not appreciably changed as regards transmission in the opposite direction. If that assumption is correct, the truth of the drainage hypothesis would seem to follow as a corollary; for it would follow that the succession of excitation B upon excitation A has resulted in the

spread of excitation from A to B along the path A-B. It would follow, at least, that the law of the attraction of the impulse from A to B is well founded for very case in which the excitation B is immediately subsequent to and stronger than the excitation process A.

It remains, then, to find positive evidence that the excitation B, in drawing to itself an impulse or current of energy from A, inhibits or in some degree diminishes the discharge of A along its own efferent channels. Now Pavlov's own language implies that this is actually the case; e. g., he writes: "Any unconditioned, or any firmly established conditioned, stimulus undoubtedly evokes a state of nervous activity in some definite part of the brain. Using the generally accepted terminology, let us refer to such areas of the brain as centres . . . . During the period of excitation of such centres (B centres) all other external stimuli which happen to affect the animal *are conducted to these centres,* and the paths by which they are conducted through the hemispheres become thereby specially marked out. This is the only possible interpretation of the facts" (p. 36).[9] Again he writes: "The facts dealt with so far reveal another important feature of this mechanism, namely, that such external stimuli as have been from the very birth of the animal transmitted to a definite centre, can, nowithstanding, be *diverted and made to follow another route,* becoming linked up by the nervous connection to another centre, *provided always that this second centre is physiologically more powerful than the first*" (p. 37).[10]

What then is the evidence that the second and stronger excitation actually diverts or drains to itself the weaker and earlier excited excitation? It consists in this chiefly—that the motor and glandular expression, the efferent outcome, of the one stimulus A (which is in process of being conditioned) is actually cut short or inhibited by the second and stronger excitation process B. The most striking instances of such inhibition are those in which a pain-exciting stimulus is converted into a conditioned stimulus of the alimentary or food-seeking reaction. Writing of instances in which such a conditioned reflex has been established, Pavlov says: "Subjected to the very closest scrutiny, not even the tiniest and most subtle objective phenomenon usually exhibited by animals under the influence of strong injurious stimuli can be observed in these dogs. No appreci-

[9]Italics mine.
[10]Italics mine.

able changes in the pulse or in the respiration occur in these animals, whereas such changes are always most prominent when the nocuous stimulus has not been converted into an alimentary conditioned stimulus. Such a remarkable phenomenon is the result of *diverting the nervous impulse* from one physiological path to another. This transference is dependent, however, upon a very definite condition—namely, upon the relative strengths of the two unconditioned reflexes. Successful transformation of the unconditioned stimulus for one reflex into the conditioned stimulus for another reflex can be brought about only when the former reflex is physiologically weaker and biologically of less importance than the latter" (p. 30).[11]

And here is a clear statement of the law of the attraction of the impulse. "It appears that the cells predominantly excited at a given time become foci *attracting to themselves the nervous impulses aroused by new stimuli*—impulses which on repetition tend to follow the same path and so to establish conditioned reflexes" (p. 38).[12]

Is it, then, too much to say that Pavlov's conclusions, in so far as they are well founded (and they are supported by a wealth of experimental observations), go far to establish the truth of the hypothesis of inhibition by drainage?

In order that the evidence shall be complete it would be necessary to find evidence pointing to positive answers to the following questions:

1) Does the excitation initiated by the stimulus to be conditioned suffer inhibition during the conditioning process, i. e., is it inhibited partially or wholly upon the incidence of the conditioning stimulus?

2) Does the inhibiting stimulus always evoke as its primary and immediate effect an excitation process in one neurone system, the inhibition of another neurone system being merely a collateral and secondary effect of the excitation process in the former?

3) If the answers to the foregoing questions are positive, is there evidence that the primary excitation process initiated by the inhibitory stimulus is in any degree intensified or augmented in consequence of the inhibition effected by it, as the drainage hypothesis would imply?

Pavlov, not having had the notion of drainage in mind as a

[11]Italics mine.
[12]Italics mine.

guiding hypothesis, has not explicitly directed his observations to throw light on these questions. But clear indications of positive answers to all of them are to be found in his pages.

In most instances nothing is said of the direct or unconditioned efferent effects of many of the stimuli used in the experiments and converted by them into conditioned stimuli of the "alimentary reflex" or salivary flow (which throughout is chiefly used as the indicator of the conditioned reflex). But in some instances it is made clear that the stimulus to be conditioned does, antecedent to the conditioning process, produce specific and in some cases widespread excitation. This is true, e. g., of the nocuous (or painful) cutaneous stimuli which in becoming conditioned cease to evoke the movements and the respiratory and circulatory effects which they evoke before conditioning. Many other of the stimuli which were conditioned (i. e., rendered capable of exciting the salivary reflex by repeatedly offering food shortly after the inception of the stimulus to be conditioned) excite what Pavlov calls "the investigatory reflex." "In a normal animal the slightest alteration in the environment—even the very slightest sound or faintest odor, or the smallest change in intensity of illumination—immediately evokes the reflex which I referred to in the first lecture as the investigatory reflex—manifested by a very definite motor reaction." And on another page (312) we are told: "In the case of a new, even small, change in the environment, two reflexes appear—a positive one, the investigatory reflex, and a negative one, which might be described as a reflex of caution and restraint." It is, then, a fair assumption that in every case the so-called neutral stimulus to which the conditioning process is applied is one which has its natural efferent effects, which effects no longer occur when the conditioning process has been completed, being replaced by those of the conditioning stimulus, namely, the salivary secretion and movements towards the food.

I have searched in vain in the pages of Pavlov's book for any descripiton of the immediate effect of the conditioning stimulus upon the efferent effects of the neutral stimulus. The course of the conditioning process is nowhere fully described, although a great number of experiments illustrating the fully conditioned reflex are cited. Yet, since the defense reactions (in the case of the nocuous stimuli) and the investigatory reactions (in case of the many mild stimuli which provoke them) no longer occur when the conditioning

of these stimuli has been completed, it is a fair assumption that their reactions are inhibited (wholly or partially) at each repetition of the unconditioned stimulus during the conditioning process. Yet it remains a matter for regret that we are not explicitly informed on this point.

As regards the second question, there is much evidence pointing to a positive answer. The conditioning stimulus most generally used in Pavlov's procedure is the presentation of food, a stimulus which provokes salivary secretion and movements towards the food. We cannot, then, generalize from this one type of stimulus and its inhibitory effect. But there are many other instances cited in which conditioned reflexes are inhibited by stimuli which evoke instinctive reactions; thus any stimulus which evokes "the investigatory reflex" is apt to inhibit any conditioned reflex. Pavlov writes: "In the case of extra stimuli which awaken specialized reflex responses, the resulting inhibition is extremely profound. Examples of such extra stimuli are the sight or sound of game for sporting dogs, of cats for some dogs, rustling under the floor for others, and so on. Not only such specialized stimuli, but also any very strong or unusual stimulus exhibits a prolonged and pronounced inhibitory effect. With respect to strong or unusual stimuli dogs can be divided into two groups. Some respond in a manner which may be termed positive, i.e., aggressively—barking furiously and baring their teeth. Others exhibit a defense reaction of a passive nature—they try to get free and run away or they stand like lumps of stone absolutely motionless—in these dogs inhibition predominates." That is to say, all stimuli which evoke the reactions of curiousity, of pugnacity, or of fear are apt to inhibit any conditioned salivary reflex; in all such cases the inhibition is clearly a secondary collateral effect of the positive excitation leading to motor activities, i.e., in all such cases the inhibition is what Pavlov calls indirect or external inhibition; and we have seen that Pavlov himself finally inclines to regard all cerebral inhibitions as of this type, there being no clear instances to the contrary, no clear instances of internal or direct inhibition, revealed by his studies as taking place in the cerebrum.

The third question is the most crucial for the drainage hypothesis. If it can be shown that the inhibiting excitation is augmented when it inhibits a prior excitation, that will be the final and conclusive proof of the drainage hypothesis. The only evidence of this kind

which I have been able to adduce from experimental observation is that afforded by graded visual contrast, in which case the inhibiting process does show augmentation in virtue of its inhibitory influence.[13] Pavlov's experiments were not designed to afford direct evidence of such augmentation. What is needed is an experiment in which the amounts of the salivary secretion produced by a given stimulus shall be compared when the stimulus immediately follows upon and inhibits the excitation effect of another stimulus and when it does not follow and inhibit the effect of another stimulus. The drainage hypothesis requires that the salivary secretion shall be greater in the former than in the latter case; and any instances in which this result might be observed would afford very strong evidence of its truth.

Although Pavlov has not designed such an experiment, we find strong indications of augmentation of an inhibiting excitation in his accounts of the phenomena of induction. In reviewing Sherrington's work on the spinal reflexes, I pointed out that positive induction as there revealed seems to imply the drainage process.[14] The similar phenomena in the cerebrum to which Pavlov applies the same name are described by him as follows: Induction is "the reinforcing effect exerted by the one process on the other . . . Induction is mutual, or reciprocal, excitation leading to increased inhibition and inhibition leading to increased excitation. The former is referred to as 'the phase of negative induction,' and the latter as 'the phase of positive induction.' " These induction effects are successive, i.e., the reinforcement or augmentation of the effect of a stimulus occurs when that stimulus follows immediately (or within a few seconds) upon the application of the other stimulus. Thus, two closely allied sense-impressions or stimuli, X and Y (each of which excites the "investigatory reflex"), are applied alternately; X is always followed by presentation of food; Y is never so followed. After a certain amount of such practice it is found that X has become conditioned (i.e., excites the salivary secretion), while Y has become differentiated (i.e., excites no trace of salivary secretion) but continues rather to excite the "investigatory reflex." If now X is applied immediately after the application of Y, X produces a considerably larger secretion of saliva than when it is not

[13] Two papers in *Proceedings of the Physiological Society,* 1903 (March and July).

[14] *Brain,* 1907, **30**.

immediately preceded by Y. We are not told explicitly that the application of X (and the consequent excitation of the conditioned reflex) inhibits or cuts short the "investigatory reflex" excited by Y; but it is a fair assumption that it does so, since the two "reflexes" have previously been shown to be incompatible, to interfere with or inhibit one another. In these instances of induction we seem, then, to have good evidence of the augmentation of the inhibiting excitation at the cost of the inhibited excitation; in other words, although not designed for this purpose, the induction experiment seems to be the *experimentation crucis* that yields the final proof of the truth of the drainage hypothesis.

It is interesting to notice that Pavlov is intrigued by the question of the relation of inhibition to his conditioning process and that, in discussing it, he comes near to formulating the drainage hypothesis, as when he writes near the end of the book: "All these observations concerning the action of the unconditioned stimulus point to the view (which involves a considerable modification of our original conception) that the mechanism of development of a conditioned reflex and the mechanism of external inhibition are somehow similar, and that the process of external inhibition bears some relation to the development of new connections between different cortical elements." Here, I say, Pavlov comes very near formulating the drainage hypothesis, for, according to this hypothesis, "the mechanism of development of a conditioned reflex and the mechanism of external inhibition" are not only "somehow similar," they are rather the one identical process viewed in two aspects, the process of drainage of the weaker excitation by the later excited and stronger excitation.

It is interesting also to note that Pavlov inclines to identify induction with the process underlying visual contrast; for as I have tried to show elsewhere all the phenomena of visual contrast, like those of induction, seem to be explicable in terms of inhibition by drainage. He writes: "There can be no doubt that the phenomena of mutual induction described above provide a physiological basis for the large group of contrast phenomena described in connection with the physiology of the sense organs" (p. 203).

I am tempted to go farther and to point out how we may apply the drainage hypothesis to interpret certain facts which Pavlov has clearly established without explaining them, especially the fact that, in order to form a conditioned reflex, in order to condition a stimulus, that stimulus must at each repetition of the conditioning process

precede the unconditioned stimulus. "If this order is reversed, the unconditioned stimulus being applied first and the neutral stimulus second, the conditioned reflex cannot be established at all." Thus, in certain experiments, the reversed order, repeated 427 times in one case and 374 times in another, failed to produce any conditioning; whereas in the same two animals, on application of the two stimuli in the favorable order (i.e., the neutral stimulus preceding the conditioning stimulus), 20 and 5 repetitions, respectively, sufficed.

Pavlov leaves this as an absolute empirical rule without attempting any explanation.

In terms of the drainage hypothesis we may see an explanation of the following general kind. If of two stimuli, X and Y, X is to be conditioned by Y, X must precede Y in order that its excitation-effect may be developed before the incidence of Y. If the onset of X in coincident with or subsequent to onset of Y, its incipient excitation is inhibited at an early point of development in the afferent channels, a point at which no considerable volume of energy has been liberated, no volume sufficient to yield a drainage current that suffices to establish a new path.

But a more satisfactory or fuller interpretation of the fact is forthcoming if we take account of a distinction which Pavlov ignores, or, in fact, repudiates, namely, the distinction between a reflex and an instinctive response. This distinction is very well founded, as I have shown at length elsewhere,[15] and I do not propose to repeat the argument here. It may suffice for the present purpose to remind the reader that an instinctive response involves a large efferent outflow in many channels, visceral and skeletal, and that this outflow seems to come in the main from some "center" in the basal ganglia. This center seems to determine the main course of the outflow, especially of the visceral outflow, and to be the main source of the energy thus directed; while the cortical circuits merely modify in detail the expression of the whole system of excitation in the skeletal muscles.

Now it is obvious that Pavlov is dealing in the main, if not exclusively, with instinctive responses, or rather fragments of such responses. His alimentary or salivary reflex is a fragment of the extensive food-seeking response of the hunger instinct, as is made clear by many of Pavlov's own statements, e.g., that its "physiolog-

[15]McDougall, W. Outline of psychology. New York: Scribner's, 1923. Pp. xvi+456.

ical strength" depends upon the degree of hunger of the animal; that as regards its "physiological strength" it stands high in the hierarchy of the reflexes; that the salivary secretion is commonly accompanied by movements of turning toward and grasping the food.

Is it not clear that if the offered food were withdrawn and the dog were not restrained, the dog would pursue it; and that, if the food took the form of a live rabbit, the "alimentary reflex" would expand itself into a vigorous pursuit of the fleeing food, a pursuit in which well nigh every motor capacity possessed by the dog might be brought into play? Surely it would seem a little arbitrary to describe such a series of movements as "a reflex"!

The inadequacy fo the term reflex, the irradiation of the self-blinding effect of the refusal to recognize the distinctive nature of the instinctive response, appear no less clearly in relation to other of the so-called reflexes. Thus, "if the males have been housed near the females (in heat) before the experiment, it is found that all their conditioned reflexes are inhibited in greater or lesser degree. It is obvious in this case that the inhibitions derive from the sexual centres in the hemispheres" (p. 47).

Again, the "investigatory reflex" (which we are told is "evoked by any minutest change in the environment") is one of the great inhibitors of conditioned reflexes. Now the movements of investigation are no doubt in many cases highly varied, but in this instance we may more effectively protest against the term "a reflex" on the ground of the multiplicity and variety of the sense-impressions which may evoke the so-called reflex movements. Surely it is of the very essence of the conception of a reflex movement that, as in the case of the knee-jerk or the scratch-reflex, it is a particular localized response to the stimulation of some particular receptor area! And when we observe a type of bodily activity that may be evoked by any and every stimulus applied to any part of any one of the sensory surfaces, we are dealing with a phenomenon whose peculiarities are wantonly obscured by applying to it the term "reflex."

Fear and anger responses are among the "reflexes" with which Pavlov is largely concerned, under the terms reflexes of passive and of active defense. His observations on the fear response are particularly interesting; they justify fully the designation I have applied to the fear instinct, namely, "the great inhibitor." Some of the dogs suffered great excitement (presumably of fear) during a

flood. Thereafter for a considerable period their conditioned reflexes could not be evoked. In one of them, some months after the event, "when the reflexes were perfectly normal," the silent admission of a trickle of water under the door of the experiment room (p. 314) sufficed to inhibit completely the long-established conditioned reflexes; "the animal jumps up quickly, gazes restlessly at the floor, tries to get off the stand and breathes heavily. The conditioned stimuli serve only to increase the general excitation; the animal declines food." Here it is clear that the single experience in the flood has set up a "conditioned reflex" which was powerful enough to inhibit not only the well-established conditioned reflexes but also the "unconditioned reflex" of taking food.

Another dog, which also had suffered the incidents of the flood, had previously acquired ten alimentary conditioned reflexes. "A week after the flood the dog was brought in to the experimental room and placed in its stand. The animal was abnormally restless and all conditioned reflexes were practically absent, and, though usually very ready for food, the animal now would not touch the food and even turned its head away" (p. 314). This persisted even when the animal was kept without food for three days; but if, instead of leaving the dog alone in the experimental room, as was the usual practice, the experimenter remained in the room, his presence sufficed to restore all the conditioned reflexes, "and the animal took the food with avidity, but it was sufficient for the experimenter to leave the animal alone for all the abnormal symptoms to recur."

Pavlov does not suggest the nature of "the reflex" produced by the presence of the experimenter, but he comments as follows on these effects of the flood experience: "It is thus seen that the powerful and unusual stimuli arising from the flood increased the susceptibility of the cortical elements to inhibition to so great an extent that even a comparatively minute intensification of inhibition from the outside, in the form of a conditioned inhibitory stimulus, rendered impossible for a long time any existence of positive conditioned reflexes"; and again, "evidently under the effect of an extremely powerful and unusual stimulus (the flood) the cortical cells, which in this dog already had a tendency to inhibition, now permanently became still more susceptible to inhibition" (p. 316). And on p. 410 he writes: "It is hardly possible not to conclude that at least in most cases what is known in psychology under the

names of 'fear,' 'cowardice,' or 'caution' has a physiological substrate in a state of inhibition of the nervous system, which varies in intensity and so produces different intensities of the reflex of passive self-protection. Developing these conceptions further we are bound to regard the obsession of fear, and different phobias, as natural symptoms of inhibition in a pathological and weakened nervous system. There are, of course, certain forms of fear, and cowardice, as for instance flight and panic, and certain postures of servility, which apparently do not conform with the idea of an underlying inhibitory process."

Here we have the most striking illustration of what I take to be the many confusions resulting from two defects of Pavlov's whole scheme of thought: first, his failure to see (or to adopt as a working hypothesis) that inhibition in the brain is always the negative collateral aspect of a process of excitation; secondly, his failure to grasp the distinction between a reflex action and an instinctive response. In the paragraph last cited he postulates an enduring change of all cortical neurones, in the direction of greater susceptibility to inhibition, as the consequence of the terrifying experience in the flood; and he makes of this increased susceptibility to inhibition the cause of the "reflex of passive self-protection" and of all the manifestations of fear and timidity that persisted for months after the flood.

I suggest that a much more plausible interpretation of the facts is the following: The fear instinct or disposition is one that, when excited, generates a very extensive and intense system of efferent outflow, one which is capable of inhibiting, by drainage, all other instinctive activities. A prolonged terrifying experience, such as the flood, leaves this disposition in a state of hypersensitivity, such that it is easily excited in great strength by many impressions which normally would excite merely the "investigatory reflex," i.e., the disposition of curiosity. The continued manifestations of excessive inhibition are then the consequence of his undue susceptibility to excitation of the fear disposition. That is to say, I suggest that Pavlov inverts the causal order in representing the cause of the excitation of various manifestations of fear to be a chronic positively inhibitory state of the cortical neurones; it is rather the chronic hyperexcitability of the fear disposition which is the ground both of the manifestations of fear and of their collateral inhibitory effects upon other activities.

Those physicians who, like myself, were charged with the care of "shell-shock" cases during the Great War are very familiar with this condition of prolonged exaggerated susceptibility to fear, revealing itself sometimes in convulsions or fits brought on by sudden noises, sometimes in the symptoms of incipient Grave's disease, sometimes in liability to terror-dreams, or in a mere general timidity. The fits are particularly interesting in this connection, since they so clearly indicate the inappropriateness of Pavlov's conception of the condition as primarily one of general intrinsic cortical inhibtion.

Coming back now to some of the special phenomena that are left by Pavlov as bare facts, we find that the conception of inhibition by drainage and the recognition of the peculiar nature of the instinctive responses enable us to interpret some of them. Let it be noted that I am not asking the physiologist to abandon his strictly physiological scheme of interpretation and to take account of any mental factors. I suggest only that the instinctive centers or dispositions must be conceived as very different from simple reflex centers. Each one, located as regards its core in the basal ganglia (*optic thalamus* principally), is capable of sending out a powerful exitatory process through a wide system of efferent channels (visceral and skeletal); such a system of channels when thus excited becomes a great open efferent path capable of draining to itself the energy liberated by many sensory stimuli and thus preventing or inhibiting their more usual efferent effects.

Apply this conception to the fact that in order to establish the conditioned reflex the neutral stimulus must precede the conditioning stimulus. If the beginning of the conditioning stimulus precedes the neutral stimulus, the latter falls on a brain in which the great alimentary channels are already open, draining to themselves all incoming currents and preventing them from reaching any other instinctive center. But if the neutral stimulus precedes by a few seconds, it has time to initiate some instinctive response, i.e., to set up a large efferent excitation; and this (if it is not too powerful) is drained or diverted to the efferent channels of the alimentary response when the alimentary stimulus sets up the more powerful outflow in that system. This is virtually a further explication of Pavlov's own explanation of the fact, for, in discussing it, he writes: "This is probably a result of external inhibition, the strong excitation produced by the unconditioned stimulus leading to inhibition

of that critical area which is but weakly excited as a result of the application of the neutral stimulus" (p. 242). Pavlov does give some slight recognition to the subcortical centers, as when he writes: "From what is known with regard to the functions of the lower parts of the central nervous system, it must undoubtedly be assumed that in the case of external inhibition of conditioned reflexes an interaction of an inhibitory character does occur between the centers of the two unconditioned reflexes."

Pavlov frequently speaks of one reflex as physiologically more powerful than another, without informing us what is his criterion of the power of a reflex. But it seems clear that his criterion is that the reflex which inhibits another is the more powerful; and it is implied that the reflexes dealt with form a hierarchy according to their relative powers of inhibition, but that each one is capable of being excited in various degrees of strength according to the nature of the stimulus and the general condition of the animal; further that they are all in relations of reciprocal inhibition with one another. Now all this seems to be unquestionably true of the instinctive responses, but not true of reflexes in general. In what then does the "physiological power" of these responses consist if not in the volume of energy which they send out along their efferent channels? The hungrier the dog, the more vigorously does he seek his food, pursue his prey, or turn to the offered food when he is on the experimental stand, and the more copious is his secretion of saliva; and also the more powerfully does this response inhibit others, the more does it resist inhibition by other responses (such as the investigatory and the defensive), and the more effective is it in "conditioning other stimuli," i.e., in setting up paths of lowered resistance converging from various points of the cortex upon its own efferent channels.

Consider now another general fact which Pavlov establishes and leaves unexplained: "Three different types of external stimuli lead to an inhibitory state of the cortex, namely, very weak stimuli, very strong stimuli, and unusual stimuli" (p. 388). That is to say, stimuli of these three classes set up excitations that are not easily inhibited and, therefore, are not easily conditioned, though by persistence under favorable circumstances they may become conditioned. Is it not the probable explanation that the weak and unusual stimuli excite strongly the "investigatory reflex," the explor-

atory response of the instinct of curiosity, while the very strong stimuli excite the fear instinct? Pavlov has shown that these two responses are not easily overcome, that they stand high in the hierarchy of "physiological power."

There are, of course, as in human beings, large individual differences among the dogs; thus Pavlov remarks that "in very inhibitable dogs the investigatory reflex exercises an extraordinarily prolonged inhibitory after-effect." And he has shown very large individual differences in respect to the general inhibitory effect of terrifying circumstances, in some dogs the effect passing off very quickly, in others a liability to inhibition of all conditioned reflexes by fear lasting for months. In the last case it is surely more consistent with probability to assume that the dogs remain in a condition in which fear is very readily excited than to assume that all the cells of the cortex remain continuously for some months in some peculiar indefinable condition indicated by the word "inhibitable."

I venture to discuss one other group of phenomena of very great general interest, namely, the relations of inhibition between the cerebral hemisphere and the lower parts of the nervous system. Pavlov reports that in many instances the dog fell asleep on the experimental stand; and this tendency was in some cases a very troublesome complication difficult to overcome. He distinguishes two kinds of sleep: one similar to the state of waxy plasticity of many hypnotized human subjects, and normal sleep. He regards the former as involving inactivity of the cortex with continued activity of the basal ganglia, the latter as involving inactivity of both cortex and basal ganglia. And he shows reason to believe that the former state normally occurs, even if only very briefly, as a stage in the onset of natural sleep.

I have always regarded the inhibitory influence of the cerebrum upon the reflexes of the spinal level as a fact very difficult to bring under the drainage hypothesis. But Pavlov's work diminishes, I think, the difficulty. It seems clear that during the waking state there are currents of activity coursing through the cortex, and the primary problem is to understand how this waking state is maintained. In the normal, healthy dog that is fed, warm, comfortable, and sheltered from all sense-impressions of a kind to evoke his instinctive tendencies, sleep seems to set in at once. A slight unfamiliar sound will evoke his "investigatory reflex"; he lifts his

head, listens, sniffs, gazes around, and sleeps again. The waking state would seem to be maintained only so long as some instinctive excitement sends up to the cortex streams of energy from the instinctive centers in the basal ganglia. Sleep would, then, seem to be the inevitable consequence of the dying-down of all instinctive excitement in the absence of exciting impressions. The same is, I think, true of man. It seems, then, hardly necessary to postulate any nervous apparatus specifically concerned in the production of sleep. Yet Claparède has put up a strong argument in favor of an instinct for sleep. If this is a reality, it must, I think, be conceived physiologically as a center in the basal ganglia which, unlike other instinctive centers, has no path of discharge to the cortex, but which, when excited, merely opens a wide efferent system of channels leading to such innervations as bring about the posture of sleep and the closing of the eyes. This, then, would be a system which, when excited, tends to drain all parts of the cortex without sending up to it any afferent stream of energy, as other instinctive centers do.

But we have no definite evidence of such a sleep-center, and Pavlov's rich new evidence on the onset of sleep tends to bear out the explanation of the onset of sleep suggested in my article on "The State of the Brain during Hypnosis,"[16] which may be concisely restated as follows: The synapses of the cortex are much more subject than those of lower levels to wide variations of their resistance. Two main factors influence their degree of resistance: first, they are very susceptible to fatigue with consequent rise of their resistance; secondly, their resistance is greatly lowered during excitation or charging of the neurones between which they lie. This susceptibility of the cortical synapses to great lowering of their resistance by excitation affords the explanation of the waking state and the inhibition of lower reflexes maintained during the waking state. The cortex, so long as it is active, is the seat of currents of energy, more or less widely diffused, which keep open within it channels of very low resistance, channels which, therefore, although they are long circuits upon the sensorimotor arcs of lower levels, are of relatively lower resistance and, therefore, drain to themselves the afferent currents from the sense organs, thus inhibiting the efferent outflow of the lower arcs. The susceptibility of cortical synapses to rise of resistance on fatigue is the main condition of

[16]Brain, 1906, and in my articles on Attention in *Mind,* 1905-1906, 11-15.

sleep. There are two principal ways in which sleep may be induced. First, general fatigue which throws into the blood an excess of fatigue products. These, acting on all cortical synapses to raise their resistance, render them no longer paths of lower resistance than the subcortical arcs; the afferent currents than follow these lower paths and are discharged without having reached the cortex, and sleep sets in as a state of general relative dissociation of the cortex. But it is the second mode of onset of sleep with which Pavlov is chiefly concerned and on which he brings much new evidence, namely, the induction of sleep by monotonously repeated stimuli. Let us notice that this is of general validity. The droning lecturer may send us to sleep at nine o'clock in the morning; the hypnotist uses monotonously repeated stimuli to induce hypnosis; the mother rocks her baby and croons a monotonous lullaby; and as Pavlov points out—"On monotonous repetition of a stimulus under constant environmental conditions puppies fall asleep very quickly and often with surprising simultaneity" (p. 258). Such stimulation is most effective in puppies, less so in grown dogs and infants, still less so in the adult man. For it seems that the susceptibility of cortical synapses to lowered resistance on excitation of the cortical neurones is a property which is relatively little developed in puppies and which reaches its maximum in the brain of the active-minded adult of the neurasthenic or introvert type, who often cannot sleep in spite of fatigue and is very resistant to hypnosis.

The monotonously repeated stimulus may be supposed to initiate an excitation that beats upon one system of cortical paths, and soon induces in the synapses of these paths a local fatigue which, blocking them and preventing the excitation from spreading through the cortex, confines it to subcortical levels. Then, in the absence of other sources of excitement, sleep follows.

Now Pavlov cites abundant evidence that the repetition of a conditioned stimulus is peculiarly effective in inducing sleep, i.e., more effective than similar repetition of a similar stimulus that has not been conditioned. A conditioned reflex to some stimulus having been established, it is found that frequent repetition of the stimulus results in a dying-away of the conditioned reflex until it ceases altogether. After an interval of rest the same stimulus is again effective in exciting the conditioned reflex. This effect is called "experimental extinction of conditioned reflexes." The ex-

tinction is the more rapid, the less well established is the conditioned reflex. Such extinction, Pavlov says, is based on internal inhibition. "In extinction the positive conditioned stimulus is temporarily transformed into a negative or inhibitory one by the simple method of repeating it several times in succession without re-enforcement" (p. 68); and, again, "the cortical cells under the influence of the conditioned stimulus always tend to pass, though sometimes very slowly, into a state of inhibition" (p. 234).

But "the longer the application of the conditioned stimulus [at each application] the quicker the development of inhibition" (p. 236); and, though re-enforcement by the conditioning stimulus may for a brief period restore the extinguished conditioned reflex, its repetition does but delay the extinction. The facts obviously point to local fatigue in the path connecting the conditioned stimulus with the unconditioned reaction as a main ground of extinction. And Pavlov himself takes this view: "the fundamental fact in all these experiments, which repeats itself time after time, is the transition sooner or later into inhibition of the state of the cortical elements acted upon by the conditioned stimulus. So far as concerns all the experimental evidence at our disposal up to the present, this transition must be regarded as depending on a functional exhaustion of the cortical elements as a result of their activity in response to a stimulus" (p. 244). Here Pavlov, as elsewhere, uses the term "inhibition" in a very wide sense to cover every kind of instance of failure of a stimulus to produce its usual effect. He insists that, though fatigue plays a part, the extinction is due to something more than a local fatigue and deserves the name of internal inhibition, because not only the particular conditioned reflex but others also and the unconditioned reflex itself often become extinguished by such repetition. However, it is open to question whether actual inhibition is at work. If we assume that the newly formed path leading the conditioned excitation over to the efferent channels of the unconditioned stimulus is, by reason of its recent formation, peculiarly susceptible to synaptic fatigue, we seem to have an assumption that suffices to explain the facts. The excitation produced by the monotonously repeated conditioned stimulus has been diverted from its own efferent route into the efferents of the unconditioned stimulus; and as the newly formed connecting path becomes blocked by fatigue, the afferent excitation fails to propagate itself in the cortex; and

this, being deprived of all afferent excitation, subsides into quiescence, a condition in which any afferent excitation has difficulty in penetrating it. This interpretation is borne out by the fact that the induction of extinction is very apt to pass over into sleep. "The fundamental condition of the appearance and development of internal inhibition [i. e., extinction] and sleep is exactly the same. It consists in the more or less prolonged or many times repeated isolated action of a conditioned stimulus producing stimulation of the cellular structures of the cortex. In all cases of internal inhibition [including extinction] which were discussed in the fourth to the seventh lectures, drowsiness and sleep were met with continually" (p. 251). It is made clear that the repetition of a conditioned stimulus is more conducive to sleep than that of any other stimulus. And this fact becomes intelligible in the light of the assumption that the newly formed path of connection between conditioned stimulus and unconditioned response is (by reason of its recent and incomplete formation) peculiarly susceptible to blocking by synaptic fatigue. Such onset of sleep appears, then, as the natural consequence of such blocking of the cortical path of the stimulus; it is a generalization to the whole cortex of the deprivation of excitation, with consequent raising of all synaptic resistances of the cortex or a general relative dissociation. Pavlov insists that the onset of sleep implies a generalization to the whole cortex of direct or internal inhibition. But we have seen that towards the end of the book he inclines to identify internal with external inhibition. I suggest, then, that if any actual inhibition is involved in the production of sleep by repetition of the conditioned stimulus, it is of the nature of external inhibition or drainage effected as follows: The excitation, finding its newly formed conditioned cortical path blocked by fatigue, discharges by some subcortical efferent system. Then occurs the stage of waxy plasticity in which postures are maintained by subcortical circuits which, by way of innervated muscles stimulating their interoceptors, keep themselves widely open. This stage Pavlov reports to be in all probability a normal incident of the passage into sleep. I can sometimes detect it in the puppy which is lying beside my chair at this moment. It occurs spontaneously in a large proportion of human subjects as they pass into deeper hypnosis. However, whether it is a normal incident or exceptional, we may fairly assume that, when it occurs, the wide open efferent channels of the subcortical circuits drain the cortex and so facilitate

the entire subsidence of excitation or of free energy within it, and the consequent state of general relative dissociation.

One other point in support of this interpretation may be made. The injection of caffeine (p. 274) was found to prevent to some extent these effects of monotonous stimulation; and an extinguished reflex can be temporarily re-established by administration of caffeine. Now, in my studies of reversible perspective, I have shown reason to believe that caffeine increases the conductivity of cortical synapses;[17] and, if this is so, it must postpone or antagonize the onset of local fatigue in the path of the monotonously repeated excitation.

Although this article is already too long, I cannot forbear to comment upon the two types of temperament among dogs which Pavlov establishes, for they go far to bear out the simple physiological interpretation of introversion and extroversion which I have suggested,[18] as against the highly complicated and vague psychological interpretations of Dr. C. G. Jung and others.

Finding trouble with the onset of sleep in the dogs during experiments, "we thought to get rid of this drowsiness by choosing for our experiments dogs which outside the experimental condition were very lively. Animals were selected which were extremely vivacious, always sniffing at everything, gazing at everything intently, and reacting quickly to the minutest sounds. Such animals when they get acquainted with men, which they do very quickly and easily, often become annoying by their continuous demonstrativeness. They can never be made to keep quiet either by order or by a mild physical punishment" (p. 285).

It would hardly be possible to improve on this as a description of the extrovert temperament in dogs, and in fits with the simple conception of extroversion which I have suggested, namely, as the expression of great freedom of outflow of excitation along all efferent channels. Now "it was soon found that these very animals when placed in the stand and limited in their movements, and especially when left alone in the experimental room, were the quickest to become drowsy, so that their conditioned reflexes quickly diminished or even disappeared altogether . . . .Indeed, our repeated stimuli,

[17]McDougall, W. The effects of fatigue and of certain drugs on the brain. London: Oxford Univ. Press, 1921.

[18]McDougall, W. Outline of abnormal psychology. New York: Scribner's. 1926. Pp. xiii+566.

when they had not yet acquired stable positive conditioned properties, immediately, and in a most unequivocal fashion, produced drowsiness and even sleep in dogs which at the commencement of the experiment were wide awake." Compare with this my generalization[19] to the effect that the extrovert subject is easily hypnotized and not subject to the neurasthenic trouble of insomnia.

Dogs of the other type are described as quiet, restrained in their movements; they react "as if most disinterested in what happens around" and "do not enter into either friendly or antagonistic relations with anybody." Pavlov writes of this as the inhibitory temperament; but it may well be regarded as the introvert temperament, the essential peculiarity of which, as I have suggested, is that excitation of the cortex spreads freely from one part to another but does not readily flow out in efferent channels. Dogs of this type do not readily fall asleep during the experiments, showing in this their affinity with the introvert human type, which is difficult to hypnotize and readily suffers from insomnia (owing to the perpetuation of excitation from part to part within the brain) and from neurasthenia with prolonged internal conflicts.

The assimilation of these two types to the extrovert and introvert temperaments is further borne out by the fact that the excitable or extrovert type is liable under prolonged attempts to induce difficult forms of conditioned reflex, to enter into a state which Pavlov describes as one of acute neurosis: "the animal began to enter into a state of general excitation. . . .and became quite crazy, unceasingly and violently moving all parts of its body, howling, barking and squealing intolerably." The other type remained undisturbed throughout such experiments. Now this outburst of wild "senseless" motor excitement of the extrovert dogs may well be likened to the hysterical outbursts to which, under strain, the human extrovert is liable.

The introvert type (Pavlov's inhibitable type) of dog, under similar conditions of prolonged strain, also enters into a state of neurosis. "The animal started at this time to lose weight and became very dull. All experiments were, therefore, interrupted for a considerable time, and the dog was given plenty of food and cod-liver oil. It soon picked up weight, and its general alertness also increased" (p. 298). It may seem far-fetched to speak of

[19] *Ibid.*

neurasthenia in a dog, but Pavlov does not scruple to describe this state as a neurosis, and from his brief description it seems analogous to the neurasthenic condition which the human introvert is liable to develop under strain.

Even as regards the condition which produces the neurosis, the parallel with human subjects holds good. It is described by Pavlov as follows: "It became obvious that under certain conditions the clashing of excitation with inhibition led to a profound disturbance of the usual balance between these two processes, and led in a greater or less degree and for a longer or shorter time to pathological disturbances of the nervous system." And again: "Broadly we can regard these disturbances as due to a conflict between the processes of excitation and inhibition, which the cortex finds difficult to resolve" (p. 302). Now, if the interpretation of inhibitory processes here maintained is correct, all these instances which Pavlov describes as "the clashing of excitation with inhibition" are in reality cases of conflict of one instinctive excitation with another. But such continued conflict is, as we now realize, the essential ground of the development of neuroses in human subjects.

We may also bring under the same scheme of interpretation the fact that in what is here called the introvert type of dog "all the types of internal inhibition develop with great ease and precision" (p. 303), and inhibition becomes readily generalized or spreads to include all cortical responses. Now this fact is in entire conformity with my interpretation of the introvert constitution; namely, that it consists essentially in such a balance of synaptic resistances that the interconnecting paths or association paths of the cortex are, as compared with the efferent paths, relatively more open or of lower resistance, whereas in the extrovert the efferent channels are the more open. For all the inhibitions with which Pavlov's observations are here concerned are inhibitions effected by transcortical paths. In the introvert nervous system, then, such transcortical inhibitions by drainage will readily be effected.

It seems in the highest degree probable that, if Professor Pavlov and his co-workers had borne in mind as a working hypothesis the conception of inhibition by drainage, they would have discovered even more striking evidences in its favor than those to which I have here drawn attention.

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## LA SIGNIFICATION DU TRAVAIL DU PROFESSEUR PAVLOV SUR LE PROBLÈME DE L'INHIBITION

(Résumé)

Dans une grande partie du travail du professeur Pavlov et de ses collègues, beaucoup en nombre, il s'agit de l'étude de l'inhibition cérébrale. Parmi les faits décrits par Pavlov lesquels soutiennent le plus directement l'hypothèse de l'inhibition par drainage, formulée par moi en 1903, sont: (1) l'inhibition cérébrale semble être dans tous les cas un effet complémentaire ou collatéral d'un procédé d'excitation dans un système de neurones et d'une décharge efférente dans un autre système; (2) les phénomènes de l'induction positive indiquent que, quand un système inhibite la décharge d'un autre système, le volume d'énergie déchargée du système qui inhibite est plus grand que dans le cas où, dans des conditions autrement les mêmes, il n'y a aucune inhibition; (3) l'inhibition de la décharge d'un système A par la plus forte excitation d'un autre système B cause la formation d'un sentier d'association A-B; (4) la répétition du procédé de l'inhibition de A par B rend ce sentier si ouvert dans la direction A à B que les excitations subséquentes de A cause la décharge du système A par les sentiers efférents du système B; (5) le stimulus, pour être conditionné, doit toujours précéder le stimulus conditionnant; ou, en autres mots, pour qu'un stimulus devienne "conditionné," il faut qu'il cause une excitation dans un système de neurones avant l'incidence du stimulus conditionnant—un procédé d'excitation dont la décharge efférente peut être inhibitée par le procédé d'excitation initié par le stimulus conditionnant subséquent. Aucuns des phénomènes de l'inhibition décrits dans la grande oeuvre de Pavlov ne semblent incompatibles avec l'hypothèse de drainage.

McDOUGALL

## DIE BEDEUTUNG VON PROFESSOR PAVLOVS ARBEIT FÜR DAS PROBLEM DER HEMMUNGEN

(Referat)

Ein grosser Teil von Professor Pavlovs Arbeit, und seiner zahlreichen Mitarbeiter, befasst sich mit dem Problem der Gehirnhemmungen. Unter den von Pavlov beschriebenen Tatsachen welche sich besonders auf die Hypothese der Hemmungen durch Drainage beziehen—die 1903 von mir aufgestellt wurde—sind die folgenden: (1) Gehirnhemmungen scheinen in allen Fällen eine begleitende oder Nebenwirkung in einem Neuronensystem eines Vorgangs der Erregung und der Entleerung in ein anderes System. (2) Die Erscheinungen der positiven Induction deuten an, dass, wenn ein System die Entleerung eines andern hemmt, die Energieintensität die von dem hemmenden System ausgschieden wird, grösser ist als wenn—unter sonst gleichen Umständen—keine Hemmung stattfindet; (3) die Hemmung der Ausleerung eines Systems A durch die stärker Erregung eines andern Systems B, erzeugt eine Assoziationsbahn A-B; (4) die Wiederholung des Hemmungsvorgangs von A durch B öffnet diese Bahn so weit in der Richtung von A-B, dass auf spätere Erregung von A eine Entleerung des Systems A durch die offenen Kanäle des B Systems erfolgt; (5) der zu bedingende Stimulus muss immer dem bedingenden Stimulus vorhergehen, oder in andern Worten, damit ein Stimulus bedingt werde, muss er in einem Neuronensystem eine Erregung auslöslen ehe die Stimulusbedingung stattfindet—ein Erregungsvorgang dessen Entladung durch den Erregungsprozess—der durch den folgenden bedingenden Stimulus hervorgerufen wird—gehemmt werden kann. Keine der Hemmungserscheinungen, die Pavlov in seinem grossen Werk beschreibt, scheinen unvereinbar mit der Entleerungstheorie.

McDOUGALL

## РАБОТА ПРОФ. ПАВЛОВА И ПРОБЛЕМА ТОРМОЖЕНИЯ.

(Реферат).

Значительная часть работы проф. Павлова и его сотрудников уделена изучению церебрального торможения. Среди фактов, описанных Павловым и приводящих непосредственно к дренажной гипотезе, сформулированной мною в 1903 году, имеются следующие: 1) церебральное торможение всегда дает дополнительный (коллатеральный) эффект в одной системе невронов и эффект возбуждения—в другой; 2) положительная индукция указывает, что когда одна система задерживает отток возбуждения от другой системы, количество энергии, оттекающей от тормозящей системы, больше, чем если торможение отсутствует; 3) торможение оттока возбуждения от одной системы *A*, происходящее благодаря наличию более сильного возбуждения системы *B*, заканчивается образованием ассоциативного пути *A*—*B*; 4) повторение процесса торможения *A* путем влияния *B* до такой степени замыкает путь *A*—*B*, что последующее возбуждение *A* вызывает отток энергии от *A* по путям, ведущим к *B*: 5) Для того, чтобы стимул стал условным, он должен предшествовать безусловному; другими словами, для этого он должен вызвать возбуждение в системе невронов до совпадения его с обусловливающим стимулом: иначе возникающий от него отток возбуждения затормозит начавшийся процесс. Ни одно из явлений торможения, описанных в огромных работах Павлова, не противоречит выдвинутой автором гипотезе дренажа.

Мак-Дауголл (McDougall)

# EMOTIONAL FACTORS IN LEARNING*

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HAROLD ELLIS JONES

The influence of emotional tone upon learning and recall is a common topic for educational theorizing and has played a leading rôle in the anecdotes of the psychoanalysts. Such principles as the law of effect, the repression of the unpleasant, and the dominance of interest in learning are related to assumptions concerning autonomic factors in behavior. Much of the evidence in this field is, unfortunately, limited to clinical observations or to test procedures which have not been supported by instrumental measurement.

A well-known study by W. Whately Smith[1] has tentatively established a curvilinear relationship between the memory value of words and their emotional significance as revealed by a galvanometer. Jung's list of 100 words,[2] slightly modified, was given to 50 subjects at Cambridge University; the psychogalvanic response was recorded for each word, and the recall for a selected portion of the list was measured at various intervals by the method of retained members. Words which yielded a high psychogalvanic deflection were characterized either by high or low memory values; the middle values for memory were held by words of indifferent emotional tone. Emotional intensity was thus considered to be either positive or negative with reference to memory; words such as *love* and *marry* were positive and well-remembered, while words such as *insult* and *afraid* were negative and repressed. This theory was further applied to the explanation of the concave form of the curve of forgetting, the rapid initial descent being imputed to the prompt loss of negatively toned memories, while the slower, later forgetting was regarded as due to the persistent retention of words with positive tone. "One conclusion of some importance. . . . .is that positive affective tone and negative affective tone are equally definite, and, so to speak,

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[1]Smith, W. W. The measurement of emotion. London: Kegan Paul, 1923. See pp. 29-44.

[2]Jung, C. G. Studies in word association. (Trans. by M. D. Eder.) New York: Moffat, Yard; London: Heinemann, 1919. See p. vii.

'real' things."[3] One does not consist merely in the absence of the other, but they represent a genuine bipolarity of emotional influence.

Certain aspects of Smith's procedure deserve comment. The association words were presented apparently in the same order for all subjects. This tends to produce spuriously high values for words which appear early in the list; Jones and Wechsler[4] have shown that the influence of position in series may be of marked importance, particularly for words of indifferent emotional tone. The operation of this factor in Smith's experiment is to be suspected, for if the P.G.R. midscores are taken for the first three groups of 10 words in his list, a diminishing series of measures is obtained: 27, 24, 20.5. Primacy is, of course, also a factor in learning, apart from the intrinsic emotional values of the words employed. It may be possible, then, for the Cambridge technique to give a spurious relationship between memory and emotional values, which would be corrected if the order of the words were rotated for different subjects. Smith has stated that one of the chief advantages of the psychogalvanic response is its objectivity in rating emotion; to some extent, however, he has sacrificed this advantage by weighting the deflections according to his judgment of their "reliability," in individual cases. The method of obtaining the memory scores is also open to some objections; each subject was mailed a list of 30 words taken from the Jung list, and instructed to commit these words to memory without the use of mnemonic aids, the list to be destroyed after one correct repetition. Reply cards were sent to each subject at successive times from 5 to 31 days, with the request that the subject write down as many words as he could recall "in two or three minutes without racking his brains." The lack of standardization in this part of the experiment may provoke criticism from those who feel inclined to question Smith's results. These results, however, are of such potential importance in psychological theory that it has seemed worth while to review the method and if necessary to repeat certain parts of the investigation.

As a check upon the Cambridge report, 16 words were taken from the upper ("critical") and lower ("non-critical") part of Smith's list, as ranked by the galvanometric deflections. The words were included in a more extended stimulus series given to 29 Columbia

[3]Smith, *op. cit.*, p. 47.

[4]Jones, H. E., & Wechsler, D. Galvanometric technique in studies of association. *Amer. J. Psychol.*, 1928, **40**, 607-612.

students, and were presented in reversed order with alternate subjects, with the purpose of compensating for the factor of P.G.R. adaptation.

Graphic psychogalvanic records were obtained by means of a specially constructed apparatus, elsewhere described.[5] The following table presents a comparison of the results in the Cambridge and Columbia experiments. The average deflections have been computed on the basis of the highest word as 100. In spite of differences in the two groups of subjects, in the method of presenting the words, and in the psychogalvanic technique, the results of the two experiments are closely similar. The rankings show a correlation of the order of .9, and agree in placement of the highest and lowest words. The Cambridge measures have a slightly greater variability, which may be due to the method employed of weighting the individual cases.

TABLE 1
A Comparison of P. G. R. Results in Two Groups

| Stimulus word | Mean deflection Cambridge | Mean deflection Columbia |
|---|---|---|
| kiss | 100 | 100 |
| love | 82 | 93 |
| marry | 80 | 90 |
| woman | 55 | 69 |
| dance | 51 | 63 |
| afraid | 50 | 79 |
| money | 48 | 66 |
| insult | 44 | 74 |
| table | 25 | 36 |
| work | 25 | 59 |
| carrot | 25 | 58 |
| white | 24 | 41 |
| glass | 24 | 46 |
| flower | 22 | 53 |
| pencil | 21 | 40 |
| swim | 19 | 29 |

The Columbia subjects being regarded as unselected for Columbia undergraduates, it was decided that for the memory tests a larger group could be used without loss of comparability.

The 16 words, preceded and followed by four buffer words, were presented serially (on large exposure cards) to four under-

[5]Wechsler, D., & Jones, H. E. A study of emotional specificity. *Amer. J. Psychol.*, 1928, **40**, 600-606.

graduate classes, totalling 100 cases.[6] The order of the words was varied in the different classes. With 50 subjects buffer words were used which bore some logical relation to words in the main series; with an equivalent 50 subjects care was taken to choose buffer words which were apparently unconnected. This experimental provision was made because of the fact that five of the positively toned words (those pertaining to sex) may be regarded as having an obvious logical relationship, and hence a high memory value, if found, might be attributed to this rather than to other factors. The two types of buffer words, however, had no differing influence upon the recall of the main series, the contrasted groups obtaining average immediate memory scores of 62 and 64% respectively. The inference seems justified (although only from analogy) that the logical relations existing among the scattered words of the main series have been of little significance in determining learning and recall; the average subject would doubtless require a more leisurely rate of learning in order to utilize such aids to organization. In the following tables, the words are ranked according to their memory values, the P.E.'s of these values being computed from Edgerton's tables.[7] The memory scores were obtained by requiring the students to write down as many words as could be recalled, immediately after one exposure of the list.

The relationships in Table 2 tend to confirm Smith's theory of the bidirectional effect of emotional tone. The three words with the highest P.G.R. values also have the highest memory values. These are sex words which may be considered as positively toned for the average subject. Two words in the list would a priori be expected to yield a negative emotional tone; *afraid* ranks fourth in P.G.R., but stands at the bottom of the list in memory; *insult* ranks fifth in P.G.R., but is one of the lowest in memory. These relationships will perhaps be clearer if we consider the differences between each of these words and each of the eight non-critical words, in terms of the chances in 100 of a true difference.

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[6]The writer is indebted to Mr. Leonard Ackerman for the collection of data on the memory tests.

[7]Edgerton, D. A., & Paterson, D. G. Table of standard errors and probable errors of percentages for varying numbers of cases. *J. Appl. Psychol.*, 1926, **10**, 378-391.

TABLE 2
A Comparison of Memory and Galvanometric Data

| Stimulus words | Percentage of recall | Mean deflections (in mm.) |
|---|---|---|
| kiss | 98 ± 1.0 | 11.46 ± 1.04 |
| love | 87 ± 2.2 | 10.69 ± .96 |
| woman | 87 ± 2.2 | 10.39 ± .97 |
| carrot | 82 ± 2.5 | 6.70 ± 1.20 |
| marry | 81 ± 2.6 | 7.96 ± .92 |
| swim | 64 ± 3.2 | 3.38 ± .33 |
| table | 59 ± 3.3 | 4.23 ± .54 |
| money | 58 ± 3.3 | 7.58 ± .69 |
| dance | 57 ± 3.3 | 7.31 ± .60 |
| pencil | 56 ± 3.4 | 4.65 ± .60 |
| work | 55 ± 3.4 | 6.85 ± 1.10 |
| write | 51 ± 3.4 | 4.69 ± .60 |
| insult | 51 ± 3.4 | 8.65 ± .78 |
| glass | 50 ± 3.4 | 5.31 ± .70 |
| flower | 41 ± 3.3 | 6.12 ± .72 |
| afraid | 32 ± 3.2 | 9.12 ± 1.23 |

TABLE 3
Chances in 100 that a Critical Word Has a Higher P.G.R. than a Compared Non-critical Word

| | flower | glass | white | work | pencil | table | swim | carrot | Av. |
|---|---|---|---|---|---|---|---|---|---|
| kiss | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 99 | 99.7 |
| love | 100 | 100 | 100 | 96 | 100 | 100 | 100 | 97 | 99.1 |
| woman | 99 | 100 | 100 | 95 | 100 | 100 | 100 | 96 | 98.7 |
| marry | 87 | 95 | 98 | 71 | 98 | 99 | 100 | 71 | 89.9 |
| insult | 95 | 99 | 100 | 91 | 100 | 100 | 100 | 83 | 96.0 |
| afraid | 92 | 97 | 98 | 84 | 98 | 99 | 100 | 86 | 94.2 |

From this table the demarcation between the critical and non-critical words is fairly evident. If the experiment had been conducted in a soundproof room, with careful attention to optimum intervals between stimuli, the reliability would presumably be near 100 in each case. It was found that the non-critical words were subject to occasional moderate deflections from extraneous factors, which, however, exerted but little effect upon words of high intrinsic emotional tone. To the extent that these disturbances have operated, they have tended to raise the non-critical average and have also affected reliabilities through the influence of increased P.E.'s.

Table 3 should now be compared with Table 4, which presents the comparison of memory values of the critical and non-critical words.

TABLE 4
CHANCES IN 100 THAT A CRITICAL WORD HAS A HIGHER MEMORY VALUE THAN A COMPARED NON-CRITICAL WORD

| | flower | glass | white | work | pencil | table | swim | carrot | Av. |
|---|---|---|---|---|---|---|---|---|---|
| kiss | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 100 |
| love | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 86 | 98.2 |
| woman | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 86 | 98.2 |
| marry | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 42 | 92.7 |
| insult | 94 | 55 | 50 | 47 | 47 | 45 | 2 | 0 | 42.5 |
| afraid | 8 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1.0 |

It may be seen from the above that the words of sexual import are sharply distinguished from the non-critical words in learning and recall as well as in emotional tone. The word *insult,* however, swings from a high position in P.G.R. to a medium or low memory value, while the word *afraid* drops to a position reliably lower, in memory, than seven of the eight non-critical words.

As the Cambridge data are not presented for the memory values of the individual words, it seemed desirable to conduct a check experiment on this part of the study. Results are available for 114 students in the University of California. With this group the 16 words, with additional buffer words, were presented as a part of a motion-picture test. The following instructions appeared on the screen:

*Word Memory. You will be shown a list of simple words. How many of them can you memorize? (Group A will learn the list at the left; Group B, the list at the right.) Ready!*

The critical and non-critical words were intermixed, and were presented in forward and reversed order for alternate groups of students. Fifty seconds were required for the exposure of the full list, and the immediate recall was tested in the same manner as at Columbia. In table 5 the words are ranked according to their memory values in the Columbia experiment.

In the comparison of the California men and women, no significant differences appear except for the word *carrot,* which is reliably better remembered by the women than by the men. The correlation between the rank orders for the two sexes is of the order of .8, which is also the measure of correspondence between the California and Columbia totals. The superiority of the Columbia group in the percentages for each word is presumably due to differences in learning ability; the Columbia median is about 1 S.D. higher than the

California median on the Army Alpha, in terms of the variability of the combined groups.

TABLE 5
PERCENTAGE OF RECALL

| | 76 California (women) | 38 California (men) | 114 California (both sexes) | 100 Columbia (men) |
|---|---|---|---|---|
| kiss | 67 | 68 | 67 | 98 |
| love | 43 | 36 | 41 | 87 |
| woman | 64 | 60 | 63 | 87 |
| carrot | 52 | 28 | 45 | 82 |
| marry | 35 | 38 | 40 | 81 |
| swim | 41 | 36 | 39 | 64 |
| table | 27 | 15 | 23 | 59 |
| money | 34 | 26 | 31 | 58 |
| dance | 41 | 52 | 44 | 57 |
| pencil | 30 | 36 | 32 | 56 |
| work | 34 | 34 | 34 | 55 |
| white | 21 | 23 | 22 | 51 |
| insult | 39 | 28 | 35 | 51 |
| glass | 26 | 21 | 23 | 50 |
| flower | 28 | 36 | 31 | 41 |
| afraid | 21 | 11 | 17 | 32 |

It will be observed that with each group the sex words have high memory values, and the word *afraid* a very low memory value. The word *insult* is also remembered more poorly than would be expected from its emotional value, if the influence of emotional tone were unidirectional. The words *carrot* and *swim* have a much higher position in the memory series than in the P.G.R. series. The writer has no explanation to offer for this, although a determined seeker after symbols might attribute it to a partially hidden sexual significance of the words, which could affect the process of memorizing without, however, becoming manifest within the stimulus-response periods of an association experiment.

## SUMMARY

1) Psychogalvanic responses and immediate memory scores were taken for a group of 16 words from Jung's list.

2) The words, as association stimuli, showed a 3 to 1 range in average P.G.R. magnitudes. A similar range ratio was found for the immediate memory values and was confirmed in a check experiment.

3) The obtained emotional and memory values represented an average for different orders of presentation; the influences of adaptation and position in series were thus eliminated.

4) On the basis of P.G.R., the words were separable into a "critical" and a relatively "non-critical" group, the rank order of the words being practically identical with that found by W. Whately Smith in a similar experiment.

5) Certain of the critical words, regarded as positively toned, yielded emotional and memory values reliably higher than those obtained for the non-critical words.[8]

6) Certain of the critical words, regarded as negatively toned, show high emotional and low memory values.

7) These results are closely similar to those found by Smith in a comparison of P.G.R. with delayed memory.

The evidence supports a theory of the bidirectional influence of emotional tone upon memory; an increased emotional intensity may affect learning either favorably or adversely, depending on whether the emotion is "positive" or "negative." It would be premature, however, to state conclusions without data from a wider range of stimuli; learning should be tested by the saving and recognition methods in addition to those previously used, and an attempt should be made to study the effects of experimental reversals of emotional tone.

Up to this time the positive-negative polarity of emotion has not been clearly defined in terms which are external to the memory experiment, although there is doubtless an implicit reference to introspective criteria of pleasantness and unpleasantness. An opinion may be offered that negative tone is related not to unpleasantness per se but rather to unpleasantness associated with reactions of shame, embarrassment, or injured self-feeling, in the common-sense meanings of these terms.

The implications of this study, for the psychology of learning, would appear to rest upon a more extended analysis of the conditions under which a directional influence may be exerted.

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[8]Cf. Würdemann, W. Ueber die Bedeutung des Gefühle für das Behalten und Erinnern. *Neue psychol. Stud.*, 1926, **1**, 511-571.

## FACTEURS ÉMOTIFS DE L'APPRENTISSAGE

(Résumé)

On a étudié le rapport entre la mémoire émotive des mots et leur signification enregistrée par un galvanomètre. On a employé seize mots, choisis sur la base des résultats antérieurs d'une expérience par W. Whately Smith. Chez un groupe de 29 étudiants universitaires, les mots, comme stimuli d'association, ont montré une étendue de 3:1 dans les magnitudes moyennes P.G.R., et ont donné une corrélation de .9 avec l'ordre de rang donné par Smith. Chez un plus grand (comparable) groupe de 100 étudiants, une pareille proportion dans l'étendue a été trouvée pour la mémoire immédiate et on a obtenu une corrélation de .8 avec les ordres de rang de mémoire dans une expérience de contrôle. On a trouvé que les mots de ton "positif," comme *amour* et *femme,* ont été en tête de la liste au point de vue de la déflexion psychogalvanique et de la valeur de mémoire. Les mots de ton "négatif," comme *insulte* et *peureux,* ont donné des réponses très émotives, mais ont possédé des valeurs de mémoire constamment plus basses que celles trouvées pour les mots de ton émotif indifférent. Il se trouve une discussion de la théorie de l'influence en deux directions du ton émotif.

JONES

## DAS GEFÜHL ALS FAKTOR BEIM LERNEN

(Referat)

Man untersuchte die Beziehung zwischen dem unmittelbaren Gedächtnis für Wörter, und deren Bedeutung in Gefühlserregungen nach den Verzeichnungen des Galvanometers. Man benutzte 16 Wörter deren Wohl durch frühere Ergebnisse eines Experimentes von W. Whately Smith bestimmt wurde. Bei einer Gruppe von 29 College Studenten zeigten die Wörter, als Assoziationsstimuli eine 3:1 Ausdehnung im Durchschnitt P. G. R. grössen, und korrelierten .9 mit der Rangardnung von Smith. In einer grösseren (vergleichbaren) Gruppe von 100 Studenten fand man eine ähnliche Rangrate für das unmittelbare Gedächtnis, und bei dem Kontrollexperiment erhielt man eine Korrelation von .8 bei der Gedächtnisrangordnung. Es fand sich, dass "positiv" betonte Wörter, wie *Liebe* (love) und *Frau* (woman), als psychogalvanische Ablenkung sowohl wie als Gedächtniswert an der Spitze der Liste standen. "Negativ" betonte Wörter wie *Insult* und *bange* (afraid) erzeugten starke Erregung, besassen jedoch entschieden geringere Gedächtniswerte als Wörter von gleichgültigem Gefühlston. Zum Schluss wird die Theorie des "bidirectional" Einflusses des Gefühlstones erörtert.

JONES

## ЭМОЦИОНАЛЬНЫЕ ФАКТОРЫ ОБУЧЕНИЯ.

(Реферат).

В настоящем исследовании изучалась связь между непосредственным запоминанием слов и их эмоциональной окраской, регистрируемой гальванометром. Было взято 16 слов, выбранных на основании результатов, полученных *W. Whately Smith*. У группы студентов, состоявшей из 29 человек, слова-раздражители дали отношение величин *P. G. R.*, равное 3:1, и давали корреляцию. 9 с рядом, приводимым Смитом. В сравнительно большей группе, состоявшей из 100 человек, подобное же соотношение было установлено для непосредственного запечатлевания; по отношению к ранговому расположению данных контрольного опыта была получена корреляция. 8. Было установлено, что „положительно" окрашенные слова, как, напр., любовь и женщина, стояли на первом месте в отношении психогальванического отклонения и в отношении памяти. „Отрицательно" окрашенные слова, как, напр., оскорбление и боязнь, давали более сильные эмоциональные реакции, но в отношении запоминаемости давали более слабые результаты, чем при индиферентных словах. В исследовании дано изложение теории влияния эмоционального тона в обоих направлениях.

**Джонс (Jones).**

# A COMPARISON OF THE HIGH RELIEF FINGER MAZE AND THE STYLUS MAZE*[1]

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During the past year the high relief finger maze was devised by Professor Miles (2) of Stanford University. The details of the construction have been given by him, and Husband (1) has used this new type of maze to investigate the elements making for complexity in human maze learning. A cursory examination indicated that this new type (which is so simple to construct) might prove useful for learning experiments in large laboratory classes, and might, if yielding learning scores equal in reliability to those obtained from the stylus maze, be substituted for it.

It seemed pertinent, therefore, to compare statistically two mazes having exactly the same pattern but differing in type, one being a stylus maze and one a high relief finger maze. This comparison has been attempted with three objectives in mind: (1) to compare the differences in difficulty of mastering the two mazes as measured by the number of trials, number of errors, and amount of time required to learn each; (2) to inquire as to the relative reliabilities of the scores made on the two types of mazes (since a priori it was impossible to say what factors had been controlled or introduced in the new type of maze which would lead to greater or less reliability of the learning scores); and (3) to determine, if possible, the degree to which the two types of mazes were measuring learning in the same way.

## APPARATUS AND SUBJECTS

Two high relief finger mazes of different patterns to be designated henceforth as Finger Maze A and Finger Maze B, respectively, were constructed by Professor Miles. These are shown in Figures

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[1]The writer is indebted to Professor Walter Miles of Stanford University for the suggestion of the problem and for his kindness in constructing the high relief finger mazes which were employed. To her colleague, Ella B. Heim, she is grateful for laboratory assistance.

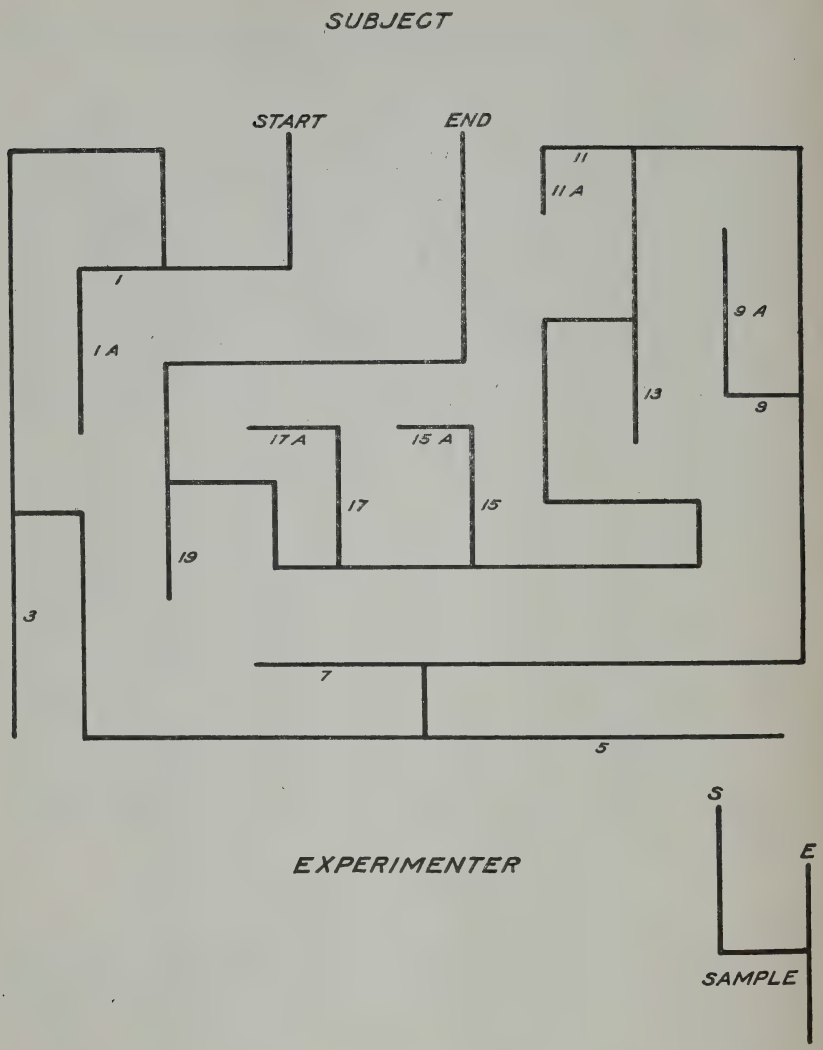


FIGURE 1
FINGER MAZE A

1 and 2. Since the method of constructing this type of maze has been given in detail in the article referred to above, a few descriptive comments will be sufficient here. All of the pathways are formed

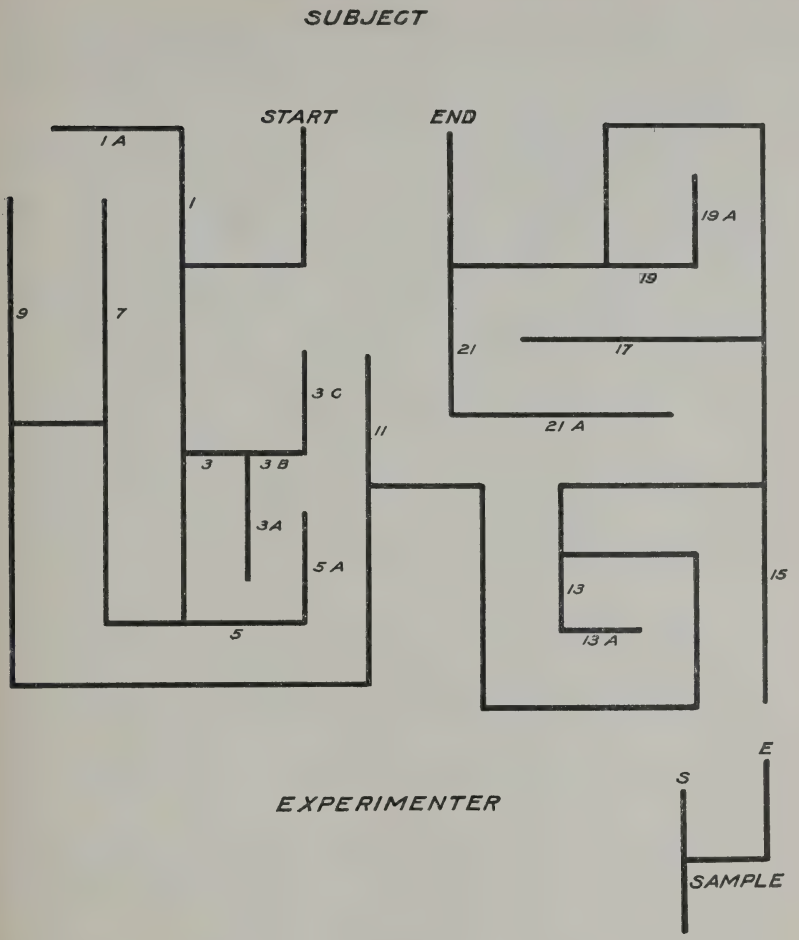


FIGURE 2
FINGER MAZE B

of No. 12 German Silver wire which, in segments, has been driven into a cedar panel 18 inches square. The units of the maze are numbered, and a coat of lacquer has been added which serves the double purpose of preserving the numbers and of giving the apparatus a smooth finish. High Relief Maze A has 10 blinds and High Relief Maze B has 11 blinds.

The two stylus mazes from which the patterns for the finger

mazes were copied were of a standard make[2] and were placed upon panels of the same size as those of the finger mazes. Drawings of the stylus mazes are not given as their sizes and patterns were exact duplicates of the corresponding finger mazes.

The blindfolds which were used on both types of mazes consisted of goggles (2) which prevented vision through having a layer of adhesive tape on the inner side of the glass. To fill in the space around the edge of the goggles and as an aid in keeping them in a sanitary condition a sheet of soft tissue with holes cut out for the eyes was inserted for each person on the inner side of the goggles.

When the results were compiled, it was found that there was a wide discrepancy in the number of subjects used in each maze. Stylus Maze A, Finger Maze A, Stylus Maze B, and Finger Maze B had, respectively, 22, 44, 26, and 40 subjects. It was originally intended to have the same number on each maze, but various factors contributed to make this impossible. Among these was the fact that the long period of time necessary in many cases to learn the stylus mazes made fewer students available during the laboratory period. Moreover, with Stylus Maze A four subjects failed to meet the criterion for learning after an average of 69.5 minutes of actual running time in the maze, and the results of their learning could not be incorporated in the calculations of the correlation coefficients. However, because it seems significant, from the standpoint of making a comparison, that four of 27 people should fail to learn this stylus maze, the means of the learning scores will be given for the group including these four subjects as well as for the group when they are omitted.

After the goggles were on the subject, the maze was placed before him. The experimenter was directed to be careful on the following points:

1) Be sure that the blindfold is properly adjusted.

2) Place the hand that is not being used in the maze-tracing at the corner of the board nearest subject.

3) Have the subject place his hand in his lap when he has finished the trial.

4) *At no time give aid of any type. Silence is imperative.*

When working with the high relief finger maze, the experimenter gave the following information to the subject:

"In this experiment you will learn the form of a raised pattern by passing

[2]Stoelting and Company, Chicago, Illinois.

your finger over it while your eyes are covered." (At this point the experimenter took the subject's index finger and placed it on the sample in the corner of the panel and illustrated the words of the next paragraph.)

"This is the starting point, a single hump. This is the finishing point, a double hump. All the paths which make up the pattern are just of this width and feel like this. They are made of nickled wire staples driven into a lacquered panel. This is a blind or jump-off place, and you feel how it comes to an end unconnected with any other path.

"Now you will make your first trial on the pattern which you are going to learn. Every one requires several trials to learn how to run through the paths perfectly, that is without going into any of the blinds. Be careful that only the tip of the index finger touches the path. As a final suggestion, remember that in our results accuracy is somewhat more important than speed."

These directions were altered for the stylus mazes by rewording the directions just given as follows:

"In this experiment you will learn the form of an inlaid pattern by passing a stylus through it while your eyes are covered." (At this point the experimenter took the subject's hand with the stylus in it and inserting the stylus at the starting-point of the maze gave some practice in the first unit.)

The following directions were substituted:

"This is the starting-place. If you return to it I will tell you. I will also tell you when you come to the end."

The remainder of the directions were as for the high relief finger maze.

The direction of the experiment was under the personal supervision of the writer at all times. In no instance were more than three mazes (i.e., three pairs of students) employed simultaneously during any one laboratory period, which made it possible to obtain conditions in the special room used for the experiment which were highly favorable for the subject and for the purposes of supervision. highly favorable for the subject was recorded in terms of three scores: number of trials to meet the criterion of learning, total number of errors made, and the number of minutes spent in actual maze running. For this last score the sum of the time scores for each of the individual trials was taken. The errors were recorded against the alley in which they were made. Thus it was possible to compare the difficulty of the corresponding blinds of mazes of the same pattern but of different types, i.e., stylus and finger.

The criterion of learning was three errorless runs in succession.

TABLE 1

TOTAL SCORES IN TRIAL, ERROR, AND TIME UNITS OF SUBJECTS FOR PATTERN *A* WITH THE MEANS AND STANDARD DEVIATIONS OF THESE SCORES

| *Stylus Maze A* | | | | *Finger Maze A* | | | |
|---|---|---|---|---|---|---|---|
| Subject | No. of trials | No. of errors | No. of min. | Subject | No. of trials | No. of errors | No. of min. |
| 1 | 22 | 104 | 49.75 | 23 | 14 | 71 | 13.67 |
| 2 | 27 | 160 | 34.50 | 24 | 14 | 29 | 22.17 |
| 3 | 49 | 283 | 84.77 | 25 | 18 | 64 | 20.08 |
| 4 | 18 | 194 | 52.98 | 26 | 20 | 54 | 22.80 |
| 5 | 38 | 244 | 31.36 | 27 | 18 | 37 | 27.96 |
| 6 | 16 | 157 | 19.91 | 28 | 14 | 100 | 23.67 |
| 7 | 20 | 122 | 26.45 | 29 | 13 | 52 | 15.87 |
| 8 | 25 | 115 | 25.25 | 30 | 11 | 34 | 20.75 |
| 9 | 45 | 119 | 37.55 | 31 | 21 | 63 | 14.46 |
| 10 | 33 | 234 | 31.60 | 32 | 11 | 42 | 16.33 |
| 11 | 25 | 159 | 39.32 | 33 | 12 | 69 | 16.40 |
| 12 | 19 | 168 | 22.96 | 34 | 22 | 155 | 48.20 |
| 13 | 18 | 151 | 27.31 | 35 | 16 | 75 | 20.10 |
| 14 | 23 | 142 | 24.80 | 36 | 19 | 59 | 20.50 |
| 15 | 11 | 72 | 6.88 | 37 | 20 | 94 | 16.10 |
| 16 | 22 | 191 | 75.52 | 38 | 14 | 83 | 21.60 |
| 17 | 43 | 306 | 64.30 | 39 | 23 | 90 | 28.08 |
| 18 | 21 | 138 | 28.66 | 40 | 13 | 64 | 23.70 |
| 19 | 22 | 317 | 33.10 | 41 | 22 | 33 | 19.30 |
| 20 | 20 | 318 | 37.30 | 42 | 19 | 54 | 27.50 |
| 21 | 28 | 235 | 34.92 | 43 | 12 | 49 | 10.52 |
| 22 | 21 | 155 | 30.96 | 44 | 9 | 39 | 3.95 |
| | | | | 45 | 26 | 100 | 27.45 |
| Mean | 25.72 | 185.64 | 37.27 | 46 | 10 | 98 | 16.70 |
| S.D. | 9.66 | 70.34 | 17.87 | 47 | 27 | 176 | 41.90 |
| N = 22* | | | | 48 | 18 | 45 | 17.05 |
| Mean | 27.68 | | 43.57 | 49 | 8 | 49 | 18.83 |
| N = 26 | | | | 50 | 22 | 98 | 31.90 |
| | | | | 51 | 14 | 78 | 26.28 |
| | | | | 52 | 15 | 53 | 12.10 |
| | | | | 53 | 29 | 86 | 28.50 |
| | | | | 54 | 24 | 33 | 34.67 |
| | | | | 55 | 26 | 59 | 35.50 |
| | | | | 56 | 13 | 69 | 15.60 |
| | | | | 57 | 11 | 129 | 20.20 |
| | | | | 58 | 25 | 163 | 29.82 |
| | | | | 59 | 13 | 49 | 30.50 |
| | | | | 60 | 17 | 160 | 22.80 |
| | | | | 61 | 16 | 189 | 25.50 |
| | | | | 62 | 33 | 160 | 27.57 |
| | | | | 63 | 33 | 87 | 35.37 |
| | | | | 64 | 20 | 131 | 34.50 |
| | | | | 65 | 15 | 103 | 33.50 |
| | | | | 66 | 15 | 114 | 13.33 |
| | | | | Mean | 17.80 | 82.70 | 23.48 |
| | | | | S.D. | 6.22 | 42.04 | 8.66 |
| | | | | N = 44 | | | |

*Four subjects were not able to meet the criterion for learning. The means of the number of trials and number of minutes when their incomplete records are included are given below in the same columns.

TABLE 2

TOTAL SCORES IN TRIAL, ERROR, AND TIME UNITS OF SUBJECTS FOR PATTERN B WITH THE MEANS AND STANDARD DEVIATIONS OF THESE SCORES

| *Stylus Maze B* | | | | *Finger Maze B* | | | |
|---|---|---|---|---|---|---|---|
| Subject | No. of trials | No. of errors | No. of min. | Subject | No. of trials | No. of errors | No. of min. |
| 67 | 23 | 169 | 12.80 | 93 | 9 | 40 | 8.91 |
| 68 | 13 | 66 | 23.10 | 94 | 6 | 15 | 11.00 |
| 69 | 17 | 117 | 30.25 | 95 | 14 | 31 | 15.80 |
| 70 | 10 | 69 | 6.28 | 96 | 26 | 57 | 21.09 |
| 71 | 11 | 86 | 18.40 | 97 | 13 | 43 | 11.25 |
| 72 | 8 | 43 | 18.10 | 98 | 9 | 16 | 16.90 |
| 73 | 38 | 202 | 31.00 | 99 | 6 | 10 | 10.13 |
| 74 | 19 | 156 | 28.00 | 100 | 8 | 22 | 13.00 |
| 75 | 18 | 174 | 41.14 | 101 | 12 | 25 | 6.40 |
| 76 | 20 | 56 | 19.38 | 102 | 12 | 28 | 6.38 |
| 77 | 36 | 186 | 44.30 | 103 | 11 | 21 | 10.80 |
| 78 | 17 | 146 | 48.49 | 104 | 9 | 23 | 10.50 |
| 79 | 10 | 68 | 26.29 | 105 | 8 | 24 | 5.83 |
| 80 | 17 | 139 | 13.05 | 106 | 11 | 62 | 33.00 |
| 81 | 12 | 12 | 18.00 | 107 | 22 | 31 | 29.50 |
| 82 | 13 | 327 | 32.13 | 108 | 11 | 41 | 10.10 |
| 83 | 11 | 40 | 4.02 | 109 | 9 | 47 | 8.25 |
| 84 | 7 | 50 | 9.90 | 110 | 11 | 27 | 7.10 |
| 85 | 15 | 179 | 30.06 | 111 | 9 | 70 | 17.27 |
| 86 | 11 | 61 | 27.60 | 112 | 14 | 32 | 9.70 |
| 87 | 15 | 56 | 16.70 | 113 | 18 | 66 | 26.48 |
| 88 | 32 | 170 | 51.00 | 114 | 12 | 78 | 16.61 |
| 89 | 6 | 8 | 5.00 | 115 | 6 | 8 | 5.02 |
| 90 | 29 | 143 | 20.08 | 116 | 19 | 112 | 35.48 |
| 91 | 19 | 70 | 17.80 | 117 | 9 | 102 | 10.67 |
| 92 | 29 | 140 | 9.60 | 118 | 28 | 35 | 14.00 |
| | | | | 119 | 24 | 58 | 19.60 |
| Mean | 17.33 | 112.80 | 22.87 | 120 | 12 | 34 | 14.00 |
| S.D. | 8.64 | 71.30 | 12.72 | 121 | 22 | 49 | 13.60 |
| N = 26 | | | | 122 | 17 | 62 | 17.10 |
| | | | | 123 | 15 | 46 | 9.49 |
| | | | | 124 | 7 | 32 | 8.67 |
| | | | | 125 | 12 | 29 | 7.00 |
| | | | | 126 | 6 | 8 | 6.15 |
| | | | | 127 | 15 | 62 | 11.00 |
| | | | | 128 | 15 | 62 | 11.00 |
| | | | | 129 | 9 | 42 | 11.35 |
| | | | | 130 | 19 | 78 | 10.40 |
| | | | | 131 | 10 | 65 | 8.20 |
| | | | | 132 | 11 | 31 | 13.70 |
| | | | | Mean | 12.88 | 42.98 | 13.32 |
| | | | | S.D. | 5.53 | 23.85 | 7.43 |
| | | | | N = 40 | | | |

## Results

*A. Comparison of the Learning Scores of the Two Types of Mazes*

In Tables 1 and 2 are presented the learning scores for each of the four mazes. From these figures it is possible to make a direct comparison of the relative difficulty in learning a given pattern in the two types of mazes, i. e., stylus and high relief. Considering first the mazes of pattern A it is seen by comparing the mean values that in the high relief finger maze a fewer number of trials is required to satisfy the criterion of learning, fewer errors are made, and less time is consumed than in the corresponding stylus maze. That these differences between the learning scores of the two mazes are really significant is shown by comparing the standard deviations of each of the measures for the two types of mazes. Especially in the error scores and time scores of the respective mazes are there found wide discrepancies in the standard deviations. Stylus Maze A has a standard deviation of 70.34 errors, and the corresponding high relief finger maze a standard deviation of 42.04 errors; the standard deviation of the time scores for the same stylus maze is 17.87 minutes, whereas the standard deviation of the time scores for the high relief finger maze is 8.66 minutes. If we go a step farther and compute a statistical constant which will give us the mathematical significance of the differences between the mean numbers of trials, mean numbers of errors, and mean time scores for the two mazes here contrasted, in terms of the standard errors of the differences, we find that these differences are real and not chance differences. These constants together with the constants of like nature for the mazes of Pattern B are given in Table 3.

TABLE 3

Means, Standard Deviations, and Standard Scores of the Differences between the Means of Similar Learning Scores for the Two Types of Mazes

| Score | Stylus Maze A N22 | | Finger Maze A N44 | | Standard score of difference |
|---|---|---|---|---|---|
| | Mean | S.D. | Mean | S.D. | |
| No. of trials | 25.72 | 9.69 | 17.80 | 6.22 | 3.491 |
| No. of errors | 185.64 | 70.34 | 82.70 | 42.04 | 6.611 |
| No. of minutes | 37.27 | 17.87 | 23.48 | 8.66 | 3.424 |
| Score | Stylus Maze B N26 | | Finger Maze B N40 | | Standard score of difference |
| | Mean | S.D. | Mean | S.D. | |
| No. of trials | 17.53 | 8.64 | 12.88 | 5.53 | 2.438 |
| No. of errors | 112.80 | 71.30 | 42.98 | 23.85 | 4.822 |
| No. of minutes | 22.97 | 12.72 | 13.32 | 7.43 | 3.500 |

From the last column of Table 3 the reliability of the differences of the means of the learning scores for the two types of mazes is readily seen. In every case except one the obtained difference is over three standard deviations from a zero difference, which indicates, if the assumption holds here of random sampling in the subjects used in the experiment, that there is a real difference in the economy of the learning scores in favor of the high relief finger maze. Thus it is possible to state with some assurance that Finger Maze B requires roughly two-thirds as many trials, one-third as many errors, and one-half as many minutes in which to be mastered as is required by Stylus Maze B. Similarly, Finger Maze A requires approximately two-thirds as many trials, two-fifths as many errors, and three-fifths as many minutes in which to be learned as is required by Stylus Maze A.

A statistical comparison has not been made of the means of the learning scores for the two mazes of pattern A when the four subjects who failed to learn Stylus Maze A are included. If the true means were available for this latter group, the differences between the means of the three scores would be certainly much greater than our figures show.

*B. Comparison of the Reliability Coefficients for the Two Types of Mazes*

Does the new type of maze per se introduce factors which make for higher or lower reliability in the learning scores? Without experimental data it is possible to suggest factors which perhaps would operate effectively in either direction. These factors may be of two types—one contingent upon the differences in the mechanical make-up of the mazes, the other dependent upon the stimuli for ideation which each affords.

Factors dependent upon the make-up of the maze are indicated by the methods used in learning the two types of mazes. In learning the stylus maze the subject with a stylus is able to have contact with but one side of the grooved pathway at a time. If he "sticks" to one side of the pathway, he may learn with great difficulty or may never learn that the "true path" branches from the other side of the groove. Successes, then, in learning the stylus maze may be wholly the result of chance successes in the choice of a technique, and thus it might be argued that the scores are not valid indices of the learning process and would probably be unreliable. On the other hand, the high relief finger maze offers the finger contact with both sides

of the path, and the obtained scores are dependent for the most part upon the factors involved in the elimination of blinds and not upon the factor of technique in attacking the problem. Thus higher reliability in the learning scores might a priori be expected in all cases from the high relief maze. A second physical difference, however, suggests that, if the high relief maze should offer a learning situation which because of its "being much easier to learn" fails to differentiate between the scores of the individuals, then we should expect learning scores obtained from it to be less reliable than those obtained from a stylus maze of similar pattern.

With respect to the differences in ideation involved in the two mazes, a priori speculation would tend toward the prediction that the high relief maze with its definite tactual stimuli would furnish a learning situation for the introduction of ideational processes far superior to that offered by the stylus maze. Whether this is justified or not cannot be learned from our data. A lack of the emotional upset and excitement which characterized the majority of the subjects on the stylus maze together with the greater smoothness of the learning curves of the high relief maze might suggest that ideation was more possible in the latter type than in the stylus, but these observations are not adduced as proof. An experiment in which the students on the two types of mazes were asked to reproduce the patterns at different stages in the learning process might afford information on this problem.

In so far as ideation, then, supplants chance factors in directing the choice of the "true path" in maze running, we would perhaps expect the high relief maze to be more reliable than the stylus maze.

To obtain some statistical material which would help to throw light on the validity of some of these assumptions, reliability coefficients were computed for one set of learning scores of each maze. The number of errors was chosen for these computations in preference to the other scores for reasons which have been argued for elsewhere (5). Because of the variability within a group in the number of trials needed to learn the maze, it was necessary to control this factor by arbitrarily cutting off the learning curves for each group at a given trial so that the scores of all individuals would be from the same segment of the learning curve. Care was taken to include all cases but to have a minimum of zero scores. The learning scores of Stylus Maze A were taken for the first 16 trials;

of Finger Maze A, for 10 trials; of Stylus Maze B, for 10 trials; of Finger Maze B, for 8 trials.

It might have been better to cut off the learning curves for the four groups at the same point, but this would have placed the point at eight trials and it was desired to include as many data as possible for each group. Thus in avoiding the danger of spurious correlation through the inclusion of zero scores, the objection of comparing reliability coefficients which have been computed from series differing in the total number of trials has been introduced. That this objection may not be serious may be assumed since a comparison is made only between the coefficients of Stylus Maze A and Finger Maze A, and Stylus Maze B and Finger Maze B, respectively, and the differences between the number of trials used for each pair is not great. Pragmatic considerations further justify the method. These differences, however, should be kept in mind in interpreting the reliability coefficients. From Figure 3, in which the learning curves for the three groups are shown, the necessity for cutting off the learning curves at definite points is evident as the respective curves at these points flatten out. Curve D has characteristics which,

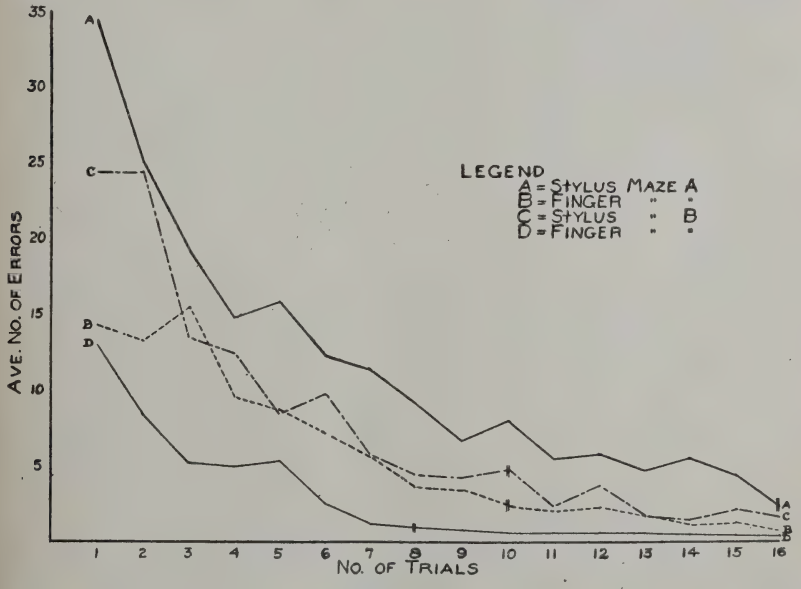


FIGURE 3
LEARNING CURVES FOR FOUR MAZES

from previous work with learning curves (5), suggested that the reliability of its scores might prove to be low.

Two of four previously described methods (4) were used in computing the reliability coefficients. The first method was that in which the sums of the errors made by each person on odd trials were correlated against the sums of the errors made on even trials within the segment of the learning curve designated above for the respective mazes. The second method consisted of correlating the sums of the errors made by each person in the first half of the segment of trials against the sums of the errors made in the second half of the segment. If a fuller discussion of the interpretations which are given to these two methods of calculating the reliability of learning scores is desired, the reader is referred to the articles on reliability cited above. In these articles the argument is supported that that maze which yields reliability coefficients which are high when calculated by the *two* methods used above is superior, for learning experiments, to the maze which yields a high reliability coefficient obtained by only one method. This contention is adduced below in our interpretation of the reliability coefficients of Table 4.

TABLE 4*

RELIABILITY COEFFICIENTS COMPUTED BY TWO METHODS FOR EACH OF THE FOUR MAZES

| | Stylus Maze A | Finger Maze A | Stylus Maze B | Finger Maze B |
|---|---|---|---|---|
| $r$ computed odd vs. even scores | .792±.031 | .729±.028 | .760±.033 | .557±.059 |
| $r$ computed first half of scores vs. second half | .247±.111 | .592±.042 | .622±.053 | .104±.102 |

*The reliability coefficients are corrected for halving the data by the Spearman-Brown formula:

$$R = \frac{2r}{1 + r}$$

The P. E.'s which are appended are computed from Shen's formula (3):

$$P.E._R = .6745 \frac{a(1 - r^2)}{\sqrt{N}\,[1 + (a-1)r]^2}$$

Two points of importance are immediately obvious from an examination of this table. First, in view of the criterion just given, Finger Maze A is superior to Stylus Maze A with respect to the

reliability of the learning scores. Secondly, Finger Maze B is decidedly inferior to Stylus Maze B with respect to the reliability of its learning scores. We have, therefore, illustrated in these two patterns (A and B) the effects on the reliability of the learning scores of changing stylus patterns into identical high relief patterns. In one case we have a more reliable maze; in the other, a more unreliable one. The important deduction here is that *the reliability of a maze depends in part upon factors other than the mode of construction (stylus or high relief) per se.* The pattern and number of effective blinds may introduce at least one other factor which, in the case of rat mazes, Tolman and Nyswander believed essential in contributing to the reliability of the maze for measuring learning, namely, the factor of complexity. A maze, if too difficult, does not permit the gradual elimination of errors by individuals. Learning, at least in the first trials, seems to be such an unintegrated process in such a maze that no differentiation can be made between the learning scores of individuals with any surety. Yet differentiation is what is desired in most experimental work. On the other hand, if a maze is too simple, again no differentiation of scores is possible.

In Finger Maze A, the too complex Stylus Maze A (i.e., for a stylus pattern) was made simpler and the resulting learning scores are more reliable. But in Finger Maze B we have a maze which is *too* simple as gauged by the number of errors made in learning it, and the learning score for an individual at any point in his learning curve is an unreliable score for differentiating him from other members of the group. In view of this preliminary work then it would seem advisable, before accepting a given maze as a reliable piece of apparatus, to ascertain first, experimentally, whether or not it is of suitable complexity and then to compute immediately its reliability coefficients before further work is done with it. Much of the work in determining the relations between various forms of sensorimotor learning and intelligence, as measured by mazes and intelligence tests, respectively, has been of little value because of the failure to determine the reliability of the maze scores.[3] Especially

[3]Professor Miles comments further on the difficulty of comparing statistically the results from learning tests and mental tests. That this a real difficulty because of the basic assumptions underlying each type of test has been pointed out to the writer by Dr. Joseph Yoshioka; namely,

1) The maze must be learned—the mental test presupposes *previous* learning.

2) Maze-running always results in a solution of the problem—in tests unsolved problems remained unsolved.

3) Reliabilities for a maze are for a *process*—for a test they are for the material.

by virtue of our findings must we be chary of assuming that the transforming of a maze pattern of known high reliability into a high relief maze will insure the same high reliability to our new maze.

It should perhaps be noted here that the patterns used in this experiment were not of the writer's choice, since the stylus mazes available determined the choice. Neither the A nor B patterns are of the T-type (5) that has been found to be best for affording reliable scores, and it is probably true that more valid comparisons of reliability would have been obtained from mazes of the T-type.

### *C. The Degree to which the Two Mazes Measure the Learning Function in the Same Way*

As previously stated, the reliability coefficients computed for each maze give indices of the consistency of the performances of the individuals who are measured on the respective mazes. Thus it would have been possible for the stylus maze and high relief finger maze to have yielded identical coefficients of reliability. This eventuality then would have meant that the mazes were equally dependable in measuring the learning function, but it would not have told us that they measured the function *in the same way,* that is, that the blinds which were most difficult in the stylus maze were the most difficult in the finger maze, or, so to speak, that there was a one-to-one correspondence between the blinds for degrees of difficulty.

In order to discover the degree to which this similarity in difficulty of blinds existed, a correlation coefficient was computed between the average number of entrances into each blind for the two mazes. The blinds were the constants. For each blind two scores were computed, one from the data on the stylus maze and the other from the data on the high relief finger maze. These scores were obtained as follows: the average number of entrances per trial into each blind was computed for each individual; the sum of these averages for each blind was averaged for the group, and this average was the score assigned to each blind. The score represents, then, the average number of entrances per trial into each blind. Scores were obtained for each blind of each of the four mazes.

The correlation between the blinds of the high relief and stylus maze of pattern A was .820±.049; of pattern B, .813±.059. These are sufficiently high to warrant the interpretation that the change in the construction of the maze pattern (i.e., stylus to high relief)

has not affected to a marked degree, as might have been assumed, the *relative* difficulty of the elements of the pattern. This is more or less surprising in view of the fact that the reliability of the scores has been materially affected in changing from the stylus to the high relief type of maze. The results, however, are not incompatible. Using pattern B as an example, it is entirely possible that the most difficult blinds and the easiest blinds, respectively, on the stylus maze were correspondingly the most difficult and the easiest on the high relief maze. But in the latter the difficult blinds were not difficult enough and the easiest blinds were too easy so that the maze was so readily learned by the entire group that no reliability can be attached to the score of any one individual as characterizing his learning ability.

### *D. Summary*

The foregoing statistical comparison of the stylus maze and the high relief finger maze has given definite information on three points; namely, their relative difficulty to learn, their relative reliability, and the degree to which the difficulty of the elements of the pattern has been altered in the transition from stylus to high relief.

The use of the high relief finger maze is more economical of laboratory time than is the stylus maze, although serving the same purpose. This was true of both patterns used in the experiment.

There is some evidence for believing that a high relief maze may be constructed to yield more reliable scores than a stylus maze of the same pattern. If the stylus maze, however, is of simple pattern, then there is a possibility of the corresponding high relief finger maze's being too simple and consequently unreliable. The reliability of new patterns of high relief mazes should be determined before experimental work is done.

The pattern of the stylus maze retains its difficult and its simple elements when it is reproduced in the high relief maze.

In view of these findings it would seem that a new and perhaps important type of laboratory apparatus has been devised, which, because of its potential ability to measure learning in a reliable and time-saving manner, should prove useful not only for general laboratory work but also for experimental purposes.

### References

1. Husband, R. W. Human maze learning on a four-section, elevated finger maze. *J. Gen. Psychol.*, 1928, **1**, 15-28.

2. Miles, W. The high relief finger maze for human learning. *J. Gen. Psychol.*, 1928, **1**, 3-14.
3. Shen, E. The standard error of certain estimated coefficients of correlation. *J. Educ. Psychol.*, 1924, **15**, 462-465.
4. Stone C. P., & Nyswander, D. B. The reliability of rat learning scores from the multiple-T maze as determined by four different methods. *Ped. Sem.*, 1927, **34**, 497-524.
5. Tolman, E. C., & Nyswander, D. B. The reliability and validity of maze measures for rats. *J. Comp. Psychol.*, 1927, **6**, 425-460.

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## UNE COMPARAISON DU LABYRINTHE À DOIGT, DE HAUT RELIEF, ET LE LABYRINTHE À STYLET

(Résumé)

Le professeur Miles de l'Université Stanford a inventé le labyrinthe à doigt, de haut relief, à employer dans l'apprentissage des expériences sur l'apprentissage du labyrinthe par les êtres humains. On a fait une comparaison entre les résultats obtenus de ce type de labyrinthe et du plus ancien type appelé le labyrinthe à stylet, avec les trois objectifs suivants: (1) de comparer les différences dans la difficulté d'apprendre les deux types de labyrinthe mesurée par le nombre d'épreuves, le nombre d'erreurs, et le temps nécessaire pour atteindre le critérium donné de l'apprentissage; (2) de déterminer les constances relatives des résultats donnés par les deux types; (3) de trouver comment les deux types de labyrinthe mesuraient l'apprentissage de la même manière.

On a employé quatre labyrinthes dans l'expérience, deux labyrinthes à stylet de forme différente et deux labyrinthes à doigt, de haut relief, lesquels ont été de la même grandeur et de la même forme que les labyrinthes à stylet. Les résultats obtenus ont montré (1) qu'il y a une différence statistiquement valide dans l'économie des résultats de l'apprentissage en faveur du labyrinthe de haut relief; (2) que la constance des résultats de l'apprentissage varie dans la transition du type à stylet au type à doigt, de haut relief, le premier donnant des coéfficients de plus grande constance, le second donnant des coéfficients de plus petite constance. Enfin, par la méthode de corrélation on a montré que la forme du labyrinthe à stylet retient ses éléments difficiles et simples quand elle est reproduite dans le type à doigt, de haut relief. Ainsi, le labyrinthe à doigt, de haut relief, mesure l'apprentissage de la même manière, à l'égard de la forme, que le labyrinthe à stylet.

Nyswander

## VERGLEICH DES STIFT MAZE (LABYRINTH) MIT DEM HIGH RELIEF FINGER MAZE (LABYRINTH MIT ERHABENEM MUSTER)

(Referat)

Das "high relief finger maze" zum Gebrauch bei Lernexperimenten, wurde von Professor Miles von der Stanford Universität erfunden. Der Vergleich der Ergebnisse dieses Apparates mit denen eines ältern, Stiftmaze hatte die folgenden Zwecke: (1) einen Vergleich der Erlernungsschwierigkeiten der zwei Typen, bemessen nach der Anzahl der Versuche und Irrtümer, und der nötigen Zeitdauer die Norm des Erlernens zu erreichen; (2) die verhältnismässige Zuverlässigkeit der mit den beiden Typen erzielten

Wertungen zu bestimmen; (3) zu ermitteln in wiefern die zwei Typen den Lernvorgang in gleicher Weise bemessen.

Man benutzte bei dem Experiment vier Apparate, zwei von verschiedenem Muster mit dem Stift und zwei mit erhabner Bahn von gleicher Grösse und gleichem Muster. Man gebrauchte von 22-44 Vpn. bei jedem der vier Apparate. Die Ergebnisse zeigten: (1) einen statistisch erwiesenen Unterschied in der Zeitersparnis der Lernwertungen zu gunsten des Labyrinths mit erhabner Bahn; (2) dass die Zuverlässigkeit der Lernwertung variiert bei dem Übergang von einem Typ zum andern, indem es in einem Falle höhere Zuverlässigkeitskoeffizienten ergiebt und im andern Falle niedrigere. Schliesslich bewies die Korrelationsmethode, dass das Muster des Stiftlabyrinths seine schwierigen und einfachen Merkmale behielt wenn es auf das andere Typ übertragen wurde. Daher folgert man dass das Labyrinth mit erhabener Bahn den Lernvorgang in Bezug auf das Muster in gleicher Weise bemisst wie das Sliftlabrinth.

NYSWANDER

## СРАВНЕНИЕ ЛАБИРИНТОВ ГРАФИЧЕСКОГО и РЕЛЬЕФНОГО ТИПА.

(Реферат).

Проф. Майльс (Miles) Стенфордского Университета сконструировал рельефный лабиринт для учебных опытов, а также для опытов с обучением людей. Сравнение результатов, полученных при работе с лабиринтом этого типа и с лабиринтом старого типа, предназначенных для карандаша, было предпринято с следующими тремя целями: 1) сравнить трудность обучения в лабиринтах обоих типов путем измерения числа опытов, числа ошибок и количества времени, служащих критерием обучения; 2) определить относительную достоверность результатов в каждом из двух типов; 3) установить степень, с которой в обоих лабиринтах измеряется обучаемость одному и тому же процессу.

Для эксперимента были использованы четыре лабиринта, два—обычного типа различных моделей и два—рельефных, повторяющих по размеру и плану лабиринты первого типа. В каждом из четырех лабиринтов было испытано от 22 до 44 лиц. Полученные результаты показали, что 1) статистически хорошо обоснованным является различие в экономности обучения, с перевесом в пользу рельефного лабиринта; 2) достоверность результатов обучения варирует при переходе от обычного типа к рельефному, давая в одном случае более, в другом—менее достоверные коэффициенты. В заключение, при помощи метода корреляций было показано, что модель графического лабиринта сохраняет как свои трудности, так и свою простоту при воспроизведении ее в рельефном лабиринте. Таким образом, рельефный лабиринт (при прочих равных условиях) измеряет обучаемость теми же путями, что и графический лабиринт обычного типа.

**Найсвондер (Nyswander).**

# PRACTICE AND IMPROVABILITY IN THE MÜLLER-LYER ILLUSION IN RELATION TO INTELLIGENCE*

*From the Psychological Laboratories of the University of Oregon*

H. R. Crosland, H. R. Taylor, and S. J. Newsom

In a previous paper[1] we showed that with 25 subjects no appreciable correlation obtains between degree of intelligence and susceptibility to the Müller-Lyer Illusion. It was shown that the correlation between any two of the six methods of presenting the Müller-Lyer figure to the subject was not the same for any other two, the methods correlating more or less highly with one another. It was also indicated that with the Müller-Lyer figure the greatest error occurs when the variable line, that enclosed by the converging wings, is set at a distance 50 mm. longer than objective equality and is placed before the right eye of the subject to be adjusted by his right hand. A suggestion, moreover, was given that there is a definite practice effect to be obtained with frequent applications of the Müller-Lyer figure, and that this practice effect may not be the same with intelligent and unintelligent subjects—only two subjects served, however, in the practice experiment. The present paper reports a continuation of the investigation, devoted to 30 practice trials given to each of 33 subjects, under a slightly different and more exacting technique than was reported in the first paper. It is the purpose of this paper to deal with the following five problems: (1) What is the relationship, if any, between the degree of intelligence and lack of susceptibility to error in judging the lengths of the main lines of a Müller-Lyer figure? (2) What will be the effects of 30 practice exercises upon the accuracy of judging such lengths? (3) What will be the effect of the relationship of intelligence to susceptibility to error upon the effects accruing from practice with the figure? (4) Can we prophesy from initial trials what will be the final accuracy in judging the lengths of the figure if some twenty-odd trials have been added? (5) Have

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[1]Crosland, H. R., Taylor, H. R., & Newsom, S. J. Intelligence and susceptibility to the Müller-Lyer illusion. *J. Exper. Psychol.*, 1927, **10**, 40-51

we employed a satisfactorily large population of subjects from whom to draw results so reliable as to be accepted without question?

The Müller-Lyer figure which we have employed was constructed in black on white cardboard and was fastened securely to a wooden supporting frame. Black passe-partout tape, 13 mm. in width, was glued to this cardboard facing so as to make a straight line across the center of the card. Diverging lines of tape of the same width of the central line, being 110 mm. in length, were arranged with an angle of 110° between them. This arrangement furnished a standard, non-adjustable, open-angle figure ($SL$) just 206 mm. in length. A sheet-metal slider, faced with the same type of cardboard as the back board, moving freely in a groove, limited the continuation of the main horizontal line and carried a pair of like converging wings. The experimenter himself adjusted this slider by means of a modified Jolly balance apparatus, affixed to the back of the board and attached to the slider by a universal attachment in such a manner that it converted a rotary adjustment movement into a linear motion of the slider. This mechanism enabled the experimenter to set the variable portion ($VL$) of the figure at any desired reading and also allowed him to readjust $VL$ to any desired length as was determined by the expressed judgment of the subject. To make the various judgments as accurate as possible, white paper circles, 6 mm. in diameter, were placed at the heads of the angles to serve as terminal reference points. The aforementioned apparatus was affixed firmly to one end of a long table, measuring 56 x 180 cm., and a head-rest with a reading lamp was attached to the opposite end of the table in such a manner that a constant distance of 170 cm. lay between the eyes of the subject and the illusion figure. The subject was, of course, seated at this end of the table and rested his chin in the chin-rest and directed his gaze to the opposite end of the table to the standard and variable lines of the figure which was on the line of vision and at right angles to it. The lamp used for illumination was a 10-watt, 120-volt, concentrated filament bulb in a white painted metal reflector, which gave as constant a light as was possible to obtain from the municipal lighting system. This light was adjusted very near and parallel to the subject's head and was directed at the illusion figure. The room was dark during each experimental session except for the light of the subject's reading lamp described above. No scheme of adapting the eyes of the subject to the darkness of the room was employed except to allow

approximately 30 seconds before the experiment began during which time the experimenter set his apparatus and gave the usual instructions. In giving these instructions an attempt was made to gain rapport with and the good will of the observers. Each of them was asked if he had ever passed judgment upon a figure of the Müller-Lyer type before; his answer was noted down, and later on in the investigation he was asked to describe certain conscious cues that he employed as a basis for determining helpful criteria to aid him in arriving at his various judgments. Every subject reported that the situation was entirely novel for him. The entire adjusting mechanism of the apparatus and the experimenter who manipulated it were hidden from the subject behind large wings of black cardboard, which had been constructed upon frames extending out from the illusion board and extending down to the table above which the cardboard was placed. Hidden from view, the experimenter dictated the following instructions:

The purpose of this experiment is to obtain the judgments of a number of subjects over a long period of time upon the comparative lengths of these two lines (pointing to *SL* and *VL*). You are to arrive at your judgment by comparing the distances between the white dots on the central horizontal line; if you think one of the lines is longer than the other you are to tell me how I am to adjust the right-hand portion of the figure (*VL*)[2] until, in your judgment, it just equals the left-hand portion of the figure (*SL*); if you think the two lines are already equal in length, state this conclusion to the experimenter. You are to be presented with this task upon 30 consecutive days, and after each of these the experimenter will take down a record of your accuracy; this record will not be revealed to you until at the end of the experiment. Are you sure that you understand the directions thoroughly?

In order to eliminate chance or snap judgments the experimenter, immediately upon the subject's stating his first affirmative answer at each judgment, supplemented the instructions by asking, "Are you sure that your judgment is exactly right?" No record was kept of the error in this first affirmative judgment but the values that were recorded were those that had been twice adjudged to be correct by

[2]This position, namely, with the variable on the right-hand side of the field of view, was selected because in our previous investigation it was discovered that maximal error occurred when the variable was situated to the right of the standard portion of the figure and 50 mm. greater than objective equality with it. This arrangement and this setting of the variable were uniformly employed throughout the present investigation.

the subject. This method encouraged each subject to do his best in each of the successive trials and enabled us to prevent his judgments from being disturbed by external or internal chance or casual factors.

The subjects who took part in the investigation were 33 male underclassmen in the University of Oregon, who had been tested by the 1925 and 1926 Series of the American Council on Education Tests. They had a range in median percentile rank in intelligence from .09 to .98, with a mean percentile rank of .59. The entire group of underclassmen, 1685 individuals, from whom they were drawn, had shown a range from .0 to .99, with a mean median percentile rank of .49. Our subjects, therefore, represent in themselves about the same range as characterized the entire group from whom they were chosen by chance.

We have instituted the following statistical computations: (1*a*) the Pearson product-moment correlation coefficient between intelligence median percentile rankings and the rankings for the mean amount of error expressed in centimeters in 30 daily judgments of the comparative lengths of the lines of the Müller-Lyer figure; (1*b*) the corrected value of the Pearson correlation ratio for the above relationship; and (1*c*) Blakeman's test for linearity of regression; (2*a*) the Pearson product-moment coefficient of correlation of the initial ability in the Müller-Lyer test represented by the first four trials and the degree of intelligence; and (2*b*) the Pearson product-moment coefficient of correlation between final ability in the Müller-Lyer test as represented by the last four trials and the degree of intelligence; (3*a*) the Pearson product-moment correlation between degree of improvement in the Müller-Lyer test, as indicated by the sum of the first four trials minus the sum of the last four trials, and the degree of intelligence; and (3*b*) the Pearson product-moment correlation between improvement as represented by the formula, $100 - \frac{100\ (\Sigma X_1+X_2+X_3+X_4)}{(\Sigma X_{27}+X_{28}+X_{29}+X_{30})}$, and the degree of intelligence; (4*a*) the Pearson product-moment correlation coefficient between initial ability and final ability in the Müller-Lyer test, with corrected $\eta$, (4*b*) and with Blakeman's test (4*c*) of linearity of regression; (5) the reliability coefficients of our data have been determined in seven ways; (6) we have represented the practice effects in all subjects pooled together in the form of learning curves in Figures 2 and 3, and in Figure 4 we have presented

learning curves for the more and the less intelligent subjects; and (7) certain discrepancies have occurred between the latter curves, in Figure 4, and certain coefficients of correlation stated elsewhere in the paper. These discrepancies we have attempted to explain by the mean deviations of the intelligence quartiles around the mean of the illusion variable, such as that of initial ability. Curves of learning were constructed for individual subjects, but since these curves superpose themselves at so many places and cut across each other in such bewildering fashion, making an almost undecipherable puzzle, it has been deemed inadvisable to publish them; consequently, we have thrown all persons into a single group in a learning curve in one instance and in the other one we have merely separated the less intelligent from the more intelligent subjects.

## Results

The raw scores of all subjects for all trials are expressed in centimeter deviations from objective equality and are represented in Figure 1.

(1*a*) The Pearson product-moment correlation coefficient between the mean amount of error occurring in the judgments of the Müller-Lyer figure and the degree of intelligence was found to be $+0.08$ with a P. E.$_r$ of $\pm 0.12$. Since $r$ here is smaller than 4 P. E.$_r$, almost no relationship can be said to exist between susceptibility to the illusion and the possession of intelligence. This confirms our finding of last year to the effect that "The subject ranking high in intelligence need not be expected also to rank high in susceptibility to illusion." (1*b*) Inasmuch as a unit of improvement in the early stages of practice may not be equal to a unit of practice in the late stages when the judgments are more nearly approaching the true lengths of the lines to be compared, it was thought reasonable to suppose that the relationship existing between the two variables of intelligence and susceptibility to the illusion was not linear in nature. Since the coefficient above was obtained by the Pearson product-moment method and since such a method is obviously not suited to the purpose of exhibiting non-linear relationships, we have employed Pearson's correlation ratio and we have found that $\eta = +0.228$ with a P. E.$_\eta$ of $\pm 0.11$ (1*c*) We have applied Blakeman's test for linearity and have obtained a value of 1.504 for $N(\eta^2 - r^2)$. It appears, therefore, that the existence of a linear relationship between intelligence

| M. P. R | Number of Trial | | | | | | | | | | | | | | | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | 13 | 14 | 15 | 16 | 17 | 18 | 19 | 20 | 21 | 22 | 23 | 24 | 25 | 26 | 27 | 28 | 29 | 30 |
| .980 | 3.7 | 3.4 | 3.9 | 3.3 | 3.2 | 3.6 | 3.2 | 2.9 | 3.0 | 2.7 | 1.8 | 2.3 | 1.3 | .4 | -.4 | -.2 | .0 | -.3 | -.2 | -1.4 | -1.6 | -1.2 | -2.0 | -1.8 | -2.0 | -2.1 | -2.0 | -2.0 | -2.0 | -2.0 |
| .975 | 3.4 | 2.7 | 2.0 | .8 | .8 | .9 | 1.1 | .9 | .8 | .9 | 1.2 | 1.1 | .9 | .7 | 1.0 | .6 | 1.1 | 1.0 | 1.1 | 1.4 | 1.1 | 1.1 | 1.2 | 1.8 | 1.5 | 1.1 | 1.1 | 1.0 | .8 | 1.3 |
| .968 | 4.5 | 3.5 | 1.9 | 1.8 | .7 | 1.0 | .8 | .7 | .0 | -.1 | .0 | -.8 | -.2 | -.2 | -.1 | -.2 | -.6 | -.6 | -.7 | -1.5 | -1.7 | -2.0 | -2.0 | -2.1 | -2.1 | -1.7 | -1.7 | -2.0 | -2.0 | -2.0 |
| .940 | 4.3 | 4.7 | 4.7 | 3.5 | 4.5 | 3.9 | 2.8 | 3.9 | 3.0 | 3.2 | 3.7 | 4.2 | 3.3 | 3.6 | 4.2 | 3.6 | 3.6 | 4.0 | 3.9 | 3.8 | 1.3 | 1.2 | .9 | 1.1 | .9 | .9 | .7 | .6 | .0 | .6 |
| .917 | 4.8 | 5.0 | 5.0 | 4.8 | 2.8 | 1.9 | 2.7 | 2.8 | .8 | .2 | .9 | 2.3 | 2.0 | 1.5 | .9 | .5 | 1.9 | 1.9 | 1.7 | 1.7 | 1.4 | 1.0 | 1.3 | .1 | .7 | .7 | .9 | 1.2 | 1.7 | 1.8 |
| .862 | 3.4 | 1.4 | 2.3 | 2.3 | 2.6 | 2.7 | 2.8 | 2.9 | 2.8 | 2.7 | 2.8 | 2.6 | 2.2 | 2.5 | 2.5 | 2.7 | 1.9 | 1.8 | 1.8 | 2.0 | 2.2 | 2.5 | 2.4 | 2.2 | 2.1 | 2.4 | 2.4 | 2.2 | 2.2 | 2.2 |
| .806 | 4.6 | 3.6 | 4.0 | 3.4 | 3.2 | 3.2 | 2.5 | 2.6 | 1.5 | 2.8 | 2.5 | 2.2 | 2.4 | 1.7 | 1.7 | 1.5 | 1.3 | 1.3 | 1.3 | 1.4 | 1.2 | .7 | .7 | .6 | 1.5 | .9 | 1.1 | 1.0 | 1.0 | 1.1 |
| .791 | 4.2 | 2.6 | 3.9 | 3.3 | 2.8 | 2.0 | 1.8 | 1.2 | 1.0 | 1.0 | 1.3 | .9 | 1.5 | 1.4 | 2.0 | 1.7 | 1.9 | 1.8 | 1.7 | 1.3 | 2.0 | 1.4 | 1.7 | 1.7 | 1.7 | 1.5 | 1.1 | .4 | .9 | .9 |
| .776 | 4.2 | 3.6 | 2.2 | 1.5 | 4.2 | 2.9 | 3.4 | 3.0 | 3.3 | 3.0 | 3.1 | 3.3 | 2.8 | 2.8 | 2.7 | 2.8 | 2.2 | 1.7 | 1.8 | 1.7 | 2.4 | 1.4 | 1.6 | 2.0 | 1.9 | 1.4 | 1.6 | 1.3 | 1.3 | 1.2 |
| .765 | 4.4 | 4.8 | 4.7 | 4.2 | 3.9 | 4.5 | 3.8 | 3.7 | 4.0 | 3.8 | 4.2 | 4.0 | 4.0 | 3.9 | 3.7 | 3.3 | 3.4 | 3.4 | 3.5 | 3.5 | 3.4 | 3.7 | 3.5 | 3.5 | 3.4 | 2.8 | 3.2 | 3.0 | 2.2 | 2.6 |
| .742 | 4.5 | 4.8 | 3.7 | 3.0 | 3.2 | 2.5 | 1.3 | 2.0 | 2.4 | 1.5 | 1.4 | 1.0 | .6 | 1.6 | 1.5 | 2.1 | 1.9 | 1.8 | 1.7 | 1.8 | 1.9 | .3 | -.5 | -1.2 | -1.5 | -1.5 | -2.0 | -2.1 | -1.5 | -1.5 |
| .716 | 4.8 | 3.8 | 3.5 | 3.2 | 3.1 | 2.6 | 3.0 | 2.3 | 1.4 | .7 | .2 | .0 | .2 | -.8 | -.8 | -.7 | -.7 | -.9 | 1.4 | 1.0 | -.3 | .0 | .0 | -.6 | -.7 | -1.8 | -1.9 | -1.6 | -1.8 | -1.9 |
| .671 | 3.9 | 3.4 | 3.8 | 3.4 | 3.5 | 3.0 | 2.7 | 2.9 | 2.5 | 3.2 | 2.3 | 2.0 | 1.0 | 1.2 | .7 | 1.3 | 1.3 | 1.3 | 1.3 | 2.4 | 2.9 | 1.7 | 2.0 | 2.0 | 1.4 | 1.0 | .1 | 2.4 | 2.3 | 1.9 |
| .633 | 6.4 | 6.6 | 5.8 | 6.8 | 6.1 | 7.2 | 6.8 | 6.2 | 5.8 | 6.4 | 6.0 | 6.3 | 6.9 | 6.3 | 5.9 | 6.1 | 6.1 | 6.0 | 5.4 | 4.2 | 4.2 | 2.8 | 2.6 | 2.7 | 1.9 | 1.7 | 2.1 | 1.7 | 1.2 | .6 |
| .615 | 3.7 | 3.2 | 3.6 | 3.3 | 3.2 | 3.0 | 3.0 | 3.0 | 3.1 | 2.9 | 2.6 | 2.7 | 2.6 | 2.7 | 3.1 | 3.0 | 3.1 | 2.8 | 2.8 | 2.8 | 2.8 | 2.6 | 2.3 | 2.5 | 2.0 | 2.5 | 3.0 | 3.1 | 3.3 | 3.1 |
| .615 | 3.4 | 3.6 | 3.4 | 2.3 | 3.4 | 3.6 | 4.4 | 2.6 | 3.3 | 2.9 | 3.1 | 2.8 | 2.8 | 2.5 | 2.6 | 1.7 | 2.2 | 1.8 | 2.2 | 1.4 | 2.5 | 2.6 | 2.4 | 2.2 | 2.9 | 3.1 | 2.4 | 2.5 | 2.3 | 2.3 |
| .596 | 4.6 | 4.6 | 4.3 | 4.0 | 3.1 | 4.4 | 3.7 | 3.0 | 2.8 | 2.2 | 2.7 | 2.4 | 2.8 | 2.5 | 2.9 | 1.9 | 2.5 | 2.5 | 1.6 | 2.1 | 1.8 | 2.1 | 2.6 | 2.3 | 2.4 | 2.1 | 2.2 | 1.5 | 2.2 | 2.4 |
| .572 | 3.7 | 3.6 | 3.8 | 3.7 | 3.1 | 3.5 | 5.0 | 4.4 | 4.7 | 4.9 | 4.1 | 3.7 | 3.0 | 3.1 | 3.4 | 3.0 | 3.5 | 3.4 | 3.6 | 3.7 | 3.4 | 2.9 | 1.2 | 1.9 | .8 | 1.8 | 1.0 | .4 | .7 | .0 |
| .531 | 1.0 | .1 | -.7 | -.6 | -.3 | -.3 | -.5 | -.3 | -.3 | -.1 | -.2 | -.1 | 1.0 | 1.0 | 1.1 | .6 | 1.6 | .5 | .0 | -.2 | -.5 | -.5 | -.3 | -.2 | -.4 | -.7 | -.6 | -.6 | -.5 | -.6 |
| .519 | 5.0 | 4.3 | 4.8 | 4.3 | 4.9 | 3.8 | 3.8 | 3.8 | 4.4 | 4.6 | 4.5 | 4.4 | 4.0 | 3.8 | 3.8 | 4.3 | 3.6 | 4.1 | 3.8 | 3.7 | 3.1 | 3.7 | 4.0 | 3.6 | 3.1 | 3.3 | 3.6 | 3.2 | 3.7 | 3.5 |
| .519 | 2.9 | 3.2 | 3.2 | 3.1 | 2.8 | 2.5 | 2.0 | 2.5 | 2.3 | 2.1 | 1.8 | 2.4 | 2.3 | 1.8 | 1.9 | 1.9 | 2.0 | 1.9 | 1.7 | 1.9 | 2.4 | 1.1 | 1.6 | 1.9 | 1.3 | 1.1 | .5 | 1.4 | .3 | 1.0 |
| .519 | 3.1 | 1.5 | 2.2 | 2.1 | 2.1 | 2.0 | 1.9 | 2.1 | .4 | 1.2 | 1.6 | 1.6 | 1.2 | .5 | .3 | .7 | .4 | .5 | .7 | .3 | .5 | .4 | .5 | .0 | .2 | .6 | 1.1 | .7 | .6 | .6 |
| .513 | 4.0 | 2.6 | 2.2 | 1.6 | 1.5 | 1.9 | 1.5 | .9 | .7 | 1.1 | 1.5 | .5 | 1.4 | 1.4 | 1.2 | 1.0 | 1.0 | .9 | .8 | .8 | .6 | .7 | .8 | .7 | .6 | .7 | .7 | .7 | .7 | .5 |
| .497 | 8.1 | 7.5 | 6.7 | 6.3 | 7.0 | 6.0 | 5.7 | 6.3 | 5.8 | 5.6 | 7.0 | 3.9 | 3.8 | 4.0 | 3.9 | 3.7 | 4.0 | 4.6 | 3.6 | 2.6 | .6 | 1.3 | 1.8 | 1.6 | 1.8 | 1.8 | .9 | 1.0 | .6 | -.1 |
| .447 | 4.5 | 1.9 | .8 | 1.6 | .9 | .7 | -.2 | -.5 | -1.0 | -1.1 | -1.4 | -1.5 | -1.1 | -1.8 | -1.3 | -.9 | -1.2 | .2 | -.8 | -.7 | -1.0 | -.3 | -.1 | -1.0 | -.5 | -1.3 | -1.2 | -1.4 | -1.0 | -1.4 |
| .395 | 4.5 | 3.9 | 4.4 | 3.7 | 4.1 | 3.7 | 3.6 | 4.1 | 3.9 | 3.4 | 3.8 | 4.0 | 3.9 | 4.0 | 4.4 | 3.7 | 4.0 | 3.4 | 3.0 | 3.6 | 3.5 | 3.6 | 3.3 | 3.4 | 3.3 | 3.4 | 3.4 | 3.3 | 3.2 | 2.7 |
| .313 | 3.0 | 3.6 | 3.4 | 3.1 | 3.4 | 2.7 | 2.8 | 2.6 | 2.1 | 1.7 | 1.2 | 1.5 | 1.5 | 1.5 | .8 | 1.1 | .0 | .3 | .8 | .1 | .3 | .1 | .3 | .5 | .7 | .4 | .5 | .1 | -.2 | .2 |
| .297 | 4.5 | 4.4 | 4.0 | 3.1 | 3.1 | 3.3 | 3.3 | 2.6 | 2.3 | 1.8 | 1.1 | .2 | .2 | -.2 | .3 | -.6 | -1.0 | -.4 | -.5 | -.3 | -.5 | -.9 | -.8 | -1.3 | .8 | .3 | .4 | .9 | .3 | .7 |
| .297 | 2.9 | 3.7 | 3.2 | 3.0 | 3.0 | 2.1 | 2.7 | 2.3 | 2.0 | 2.3 | 2.3 | 1.9 | 3.7 | 2.3 | 2.4 | 2.5 | 3.0 | 2.6 | 2.5 | 2.3 | 2.5 | 2.0 | 2.2 | 2.1 | 1.5 | 1.5 | 1.5 | 1.2 | 1.1 | 1.1 |
| .286 | 4.0 | 4.1 | 3.9 | 3.2 | 3.9 | 3.7 | 4.4 | 3.3 | 3.7 | 3.1 | 3.8 | 3.6 | 2.9 | 2.7 | 2.1 | 1.9 | 1.6 | 1.1 | 1.5 | .7 | 1.0 | .8 | .8 | .6 | .5 | .7 | .4 | .4 | .4 | .3 |
| .230 | 3.2 | 3.4 | 1.9 | 3.2 | 4.1 | 3.4 | 2.8 | 2.4 | 2.7 | 3.0 | 2.2 | 1.5 | 1.3 | 2.4 | 1.0 | 1.3 | 1.3 | .9 | 1.2 | 1.3 | 2.0 | 1.1 | .9 | .7 | .4 | .2 | .3 | .4 | 1.1 | .4 |
| .146 | 4.9 | 4.5 | 4.4 | 3.6 | 4.6 | 4.7 | 4.8 | 4.3 | 4.7 | 4.6 | 5.0 | 4.7 | 4.6 | 4.7 | 4.8 | 4.8 | 4.5 | 5.0 | 4.7 | 4.5 | 4.7 | 4.6 | 4.5 | 5.0 | 4.7 | 4.7 | 5.0 | 4.7 | 4.7 | 4.8 |
| .089 | 5.0 | 4.0 | 4.0 | 3.7 | 3.0 | 2.9 | 3.4 | 3.2 | 3.8 | 2.9 | 3.0 | 3.4 | 3.1 | 3.2 | 2.6 | 3.5 | 3.1 | 3.4 | 3.4 | 3.1 | 2.5 | 2.3 | 1.9 | 1.5 | 1.7 | 2.8 | 2.0 | 2.8 | 2.3 | 2.5 |

FIGURE 1
TABLE OF GROSS SCORES—ALL SUBJECTS
(Cm. error units)

and susceptibility to the Müller-Lyer illusion is established and that such a correlation is remarkably low. Also it appears that,

since the value of $\eta$ represents a probable upper limit to be expected for the correlation, we are justified in concluding that possibly some very small but negative relationship may exist between high intelligence and high susceptibility to illusion.

It should be said with respect to the errors which our subjects have exhibited in their perceptions and judgments of the figure that such errors in certain individuals fall into two sorts, namely, errors in the nature of *VL*'s being underestimated and consequently being placed in a position beyond objective equality and, secondly, *VL*'s being overestimated in the late trials in the nature of being set at a point under objective equality. Consequently, in Figure 1, such errors have been designated as *plus* and *minus* errors in centimeter deviations from objective equality. We have deemed it advisable to consider either sort of error to be a positive error regardless of its sign. This, of course, means that to overcorrect is regarded as being as much inaccurate as to undercorrect the variable portion of the figure.

(2*a*) Expressing the initial ability of our subjects in the Müller-Lyer experiment as $(\Sigma X_1+X_2+X_3+X_4)$ and then calculating the Pearson product-moment coefficient of correlation with it and the degree of intelligence, we obtain an $r$ of $+0.046$ with a P. E.$_r$ of $\pm 0.12$. This small value of $r$ also indicates that no special significance should be attached to the observation that high intelligence correlates with a slight positive value with high initial accuracy. This fact we shall discuss later in the paper in connection with learning curves presented in Figure 4. (2*b*) Expressing final ability in the Müller-Lyer trials by $(\Sigma X_{27}+X_{28}+X_{29}+X_{30})$ and computing the Pearson product-moment coefficient of correlation between it and the degree of intelligence, we obtain an $r$ of $+0.073$ with a P. E.$_r$ of $\pm 0.12$. Here again we are forced to conclude that no appreciable correlation between intelligence and susceptibility to the illusion, in even a protracted series of practice periods, exists. How reliable our data for the final practice periods are we shall ascertain in a later section of the paper.

(3) In attempting to find out if any significant relationship exists between ability to improve by pratice in the Müller-Lyer experiment and intellectual ability, we have recourse to two procedures, namely, by defining the degree of improvement as being the difference

between the sum of the last four trials and the first four trials, or by expressing improvement by the formula

$$100 - \left[ \frac{100\ (\Sigma X_1 + X_2 + X_3 + X_4)}{(\Sigma X_{27} + X_{28} + X_{29} + X_{30})} \right].$$

We shall employ both methods. (3*a*) Defining improvability as $(\Sigma X_1 + X_2 + X_3 + X_4) - (\Sigma X_{27} + X_{28} + X_{29} + X_{30})$, and computing a Pearson product-moment coefficient of correlation between it and the degree of intelligence, we obtain an $r$ of $-0.028$ with a P. $E_r$ of $\pm 0.12$. This value of $r$, while it is so small as to be practically negligible, still can be interpreted to mean that subjects ranking high in intelligence tend to improve with practice slightly less than do subjects of lesser intelligence. And (3*b*) expressing the degree of improvement by the formula,

$$100 - \left[ \frac{100\ (\Sigma X_1 + X_2 + X_3 + X_4)}{(\Sigma X_{27} + X_{28} + X_{29} + X_{30})} \right].$$

and computing a Pearson product-moment coefficient of correlation between it and the degree of intelligence, we obtain an $r$ of $-0.036 \pm 0.12$. Here, likewise, it can be said that $r$ is negligibly small, even though it suggests a slight negative relationship between high intelligence and high improvability.

It would be of interest to find out what degree of correlation obtains between the above two definitions of the degree of improvement in the Müller-Lyer test, but, since such a correlation will also represent the reliability of the data, we shall reserve such a comparison for a subsequent section of the paper.

(4*a*) Expressing initial ability and final ability by the values $(\Sigma X_1 + X_2 + X_3 + X_4)$ and $(\Sigma X_{27} + X_{28} + X_{29} + X_{30})$, respectively, and computing a Pearson product-moment correlation coefficient between them, we obtain an $r$ of $+0.204$ with a P. $E._r$ of $\pm 0.11$. At best, the value of $r$ here suggests a very low degree of relationship between initial and final ability. (4*b*) When we compute the value of $\eta$, however, we obtain an $\eta$ of $+0.452$ with a P. $E._\eta$ of $\pm 0.09$. This value of $n$ is of some significance but it best represents the probable upper limit of the positive relationship between initial and final ability and suggests that a prophecy of final ability based upon knowledge of initial ability would be extremely hazardous. (4*c*) When we test the relationship shown in *a* and *b* for the linearity of regression, by Blakeman's test, we obtain a value of 3.3396 for N $(\eta^2 - r^2)$ for the regression $YX$ and a value of 6.2832 for

the regression *XY*. It is clear that both values lie well within the limits of 11.37; this fact, therefore, suggests that the regression is linear in nature (Kelley's test for linearity has also been applied but, on account of the relatively small number of cases, has been found to be useless in this instance). Hence, our results, under *a*, *b*, and *c*, suggest a small positive relationship between initial and final abilities and, since only the probable upper limits are indicated, it may be argued that it is difficult to predict final success in the experiment. The estimated upper limit of the common factors between the initial and the final standings we have found to be only 28.95%. Such a small percentage of common factors indicates that very small credence can be placed in the initial trial records as a basis for prognosticating final success.

(5) We have computed the reliability of our data in seven fashions: (5*a*) by obtaining the Pearson rank differences $\rho$ between the two methods of defining improvement in the Müller-Lyer experiment, that is, we have worked out the rank-difference correlation between the data as expressed by the definitions previously mentioned under 3*a* and 3*b*; (5*b*) by computing the intercorrelations between the first four trials; (5*c*) by computing the correlation between the first and third as against the second and fourth trials; (5*d*) by computing with the use of the Brown-Spearman formula what would be the correlation between the first four trials and four other trials had we obtained at the same time four other trials under similar conditions of the experiment; (5*e*) by computing the intercorrelations between the last four trials; (5*f*) by computing the correlation between the 27th and 29th trials as against the 28th and 30th trials; and (5*g*) by computing with the Brown-Spearman formula what would be the correlation between the last four trials and four other trials under similar conditions had we done the other four trials at this time.

(5*a*) We find $\rho$ to equal $+0.9078 \pm 0.02$. This value of $\rho$ would ordinarily suggest a fairly high reliability of our data, but when we convert this value of $\rho$ into an estimated $r$ and when we calculate Kelley's coefficient of alienation, we find that the two measures of improvement have only 68.7% of factors in common. This latter fact suggests that the two methods measure fairly diverse aspects of improvement. But since intelligence has been shown to have such a small relationship to susceptibility to the illusion, the smallness of the common factors between the two

methods of measuring improvement in the Müller-Lyer experiment would no more than confirm the conclusion that only a very small relationship exists between intelligence and the ability to improve or to become less susceptible to the Müller-Lyer illusion. In fact, it has been suggested that the more intelligent subjects actually improve slightly less than do the less intelligent subjects. (5*b*) When we compute the intercorrelation coefficients between the first four trials we obtain *r*'s of the following values:

$r_{1,2} = +0.821 \pm 0.04$ $\quad$ $r_{2,3} = +0.883 \pm 0.03$
$r_{1,3} = +0.774 \pm 0.05$ $\quad$ $r_{2,4} = +0.860 \pm 0.03$
$r_{1,3} = +0.776 \pm 0.05$ $\quad$ $r_{3,4} = +0.925 \pm 0.02$

It is quite clear from these values that the reliability of the experiment is great even though we have employed only 33 subjects. It is wise, however, to ascertain the reliability by other means.

(5*c*) By combining the first and third trials and by then computing the correlation between such a sum and the sum of the second and fourth trials we obtain an *r* of $+0.933 \pm 0.02$. Again it is quite clear that we have obtained a very high reliability. So, perhaps, no objection can be urged against the smallness of the population with which we conducted the experiment. (5*d*) By employing the Brown-Spearman prophecy formula we are enabled to compute an estimated *r* between the sum of the first four trials and the sum of four other trials had these four additional trials been done at the time that the obtained four occurred; by doing this we obtain an estimated *r* of $+0.967 \pm 0.016$. This latter *r* represents perhaps the highest relationship possible to obtain between various trials with the Müller-Lyer illusion, and, since our obtained *r*'s are not greatly at variance with this estimated *r*, we are justified in assuming that our data here are extremely reliable. (5*e*) By computing the intercorrelations between the 27th, 28th, 29th, and 30th trials, we obtain *r*'s of the following values:

$r_{27,28} = +0.945 \pm 0.01$ $\quad$ $r_{28,29} = +0.914 \pm 0.02$
$r_{27,29} = +0.883 \pm 0.03$ $\quad$ $r_{28,30} = +0.968 \pm 0.01$
$r_{27,30} = +0.938 \pm 0.01$ $\quad$ $r_{29,30} = +0.932 \pm 0.02$

Again it appears that we have obtained from the final trials data which are remarkably reliable, but we shall envisage reliability in still other fashions. (5*f*) Taking the sum of the 27th and 29th trials and computing the *r* between this sum and the sum of the 28th and 30th trials, we obtain an *r* of $+0.968 \pm 0.01$; this *r* is exactly of the same value as the *r* which was obtained between the 28th and 30th trials and is approximately the same *r* which we have

obtained under (5*d*) above. Here again the data appear to be very reliable. And (5*g*) employing the Brown-Spearman prophecy formula and computing an *r* between the last four trials and four assumed additional trials, on the same days, we obtain an esimated *r* of $+0.984 \pm 0.01$, probably the highest *r* which could be obtained. It appears, therefore, that our 33 subjects have comprised a population sufficiently large for the purposes of this investigation.

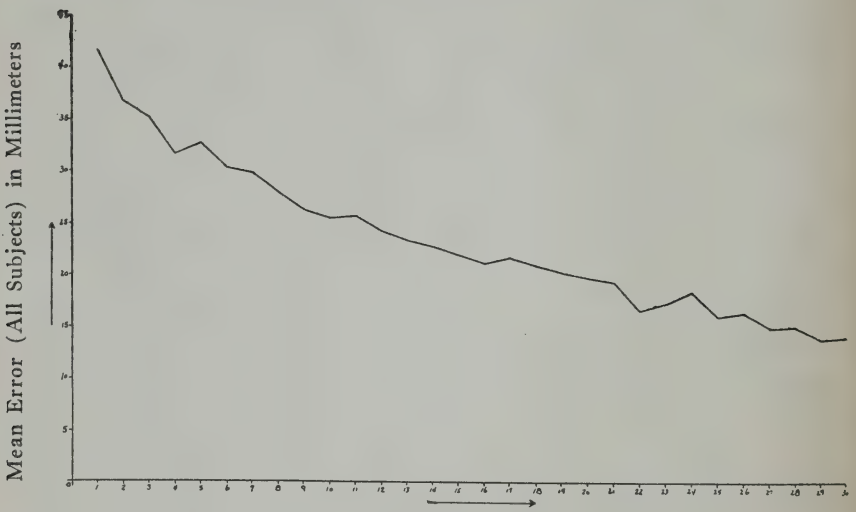


FIGURE 2
GENERAL LEARNING CURVE
Consecutive trials—all subjects

(6) Figure 2 represents the records of all 33 subjects pooled together and represents improvement, by millimeter reduction in error, throughout 30 consecutive trials in the Müller-Lyer illusion. One will note from this figure that the curve approximates roughly a true learning curve, except perhaps in not falling as rapidly as does the usual curve and by not indicating the final attainment of as high an efficiency as do the curves under the usual conditions of learning. It must not be forgotten that this is a composite curve, and for this reason perhaps the records of some subjects counterbalance the records of other subjects so that the curve does not drop as precipitously in its initial slope and does not arrive at a sufficiently low level on the graph as it should. It is true that

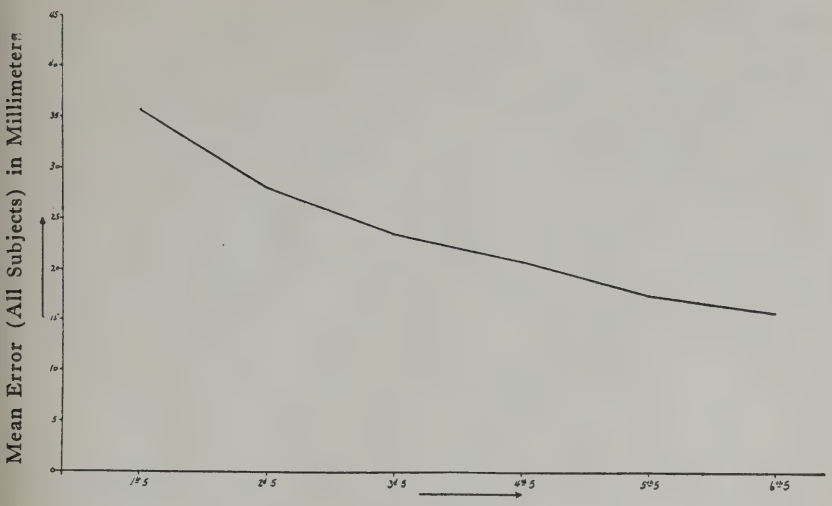


FIGURE 3
GENERAL LEARNING CURVE
Trials grouped in fives—all subjects

the individual curves start at a somewhat common point in the graph but thereafter spread almost to the four winds to such a degree that it was deemed advisable not to show them. Figure 3 represents the same facts as contained in Figure 2, except that the data are represented in means, of five trials each, giving a curve which approaches the form of a straight line. And Figure 4 represents the learning curve of the more intelligent subjects as compared with the learning curve of the less intelligent subjects. It is to be noted in Figure 4 that the lower 50 per cent of the group of subjects attain a slightly higher degree of efficiency in the Müller-Lyer test than does the upper 50–per-cent group. Likewise, the lower 50–per-cent group began the test with a higher degree of efficiency than did the upper 50–per-cent group; otherwise the two curves are very, very similar and are very closely adjacent one to the other.

And (7) the learning curves depicted in Figure 4 for the more and the less intelligent subjects would seem to suggest a slight advantage in improvement as possessed by the less intelligent subjects. This implication is at variance with the results which were obtained last year with two subjects, one of whom was far superior

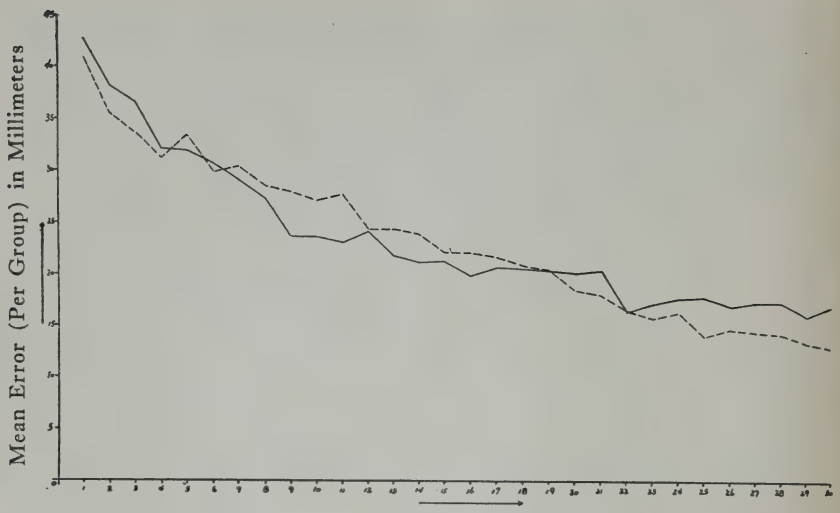


FIGURE 4
LEARNING CURVES—SUPERIOR AND INFERIOR INTELLIGENCE
Dotted line—lower 50% group
Continuous line—upper 50% group

in intelligence to the other, and conflicts with the correlation coefficients which we have presented in the early part of this paper. How can we explain this apparent conflict between obtained correlation coefficients and the graphic representation of the progress of the two groups of subjects in our experiment? If we will observe the raw data, as contained in Figure 1, we shall discover that a larger percentage of more intelligent subjects overcorrected the variable portion of the Müller-Lyer figure than was true of the less intelligent subjects. Therefore, if such overcorrections are regarded as being errors, this fact will in the late stages of practice cause the learning curve for the more intelligent subjects to rise rather than fall, which condition apparently prevails in the curves in Figure 4. We have gone farther in attempting to explain this inconsistency. We have taken the pains to compute the average intelligence median percentile ratings for the two groups of subjects, one group of whom consistently overcorrected, that is underestimated the length of *VL,* the other group of whom consistently undercorrected, that is, overestimated the length of *VL.* We find that the 11 subjects who approached no nearer than 1 cm. to the true

value of $VL$ have a mean median percentile ranking of .516; and that the six subjects who consistently overcorrected $VL$ by as much as 1 cm. or more have a mean median percentile ranking of .692. This fact rather supports the explanation just given of the seeming divergence between the facts of Figure 4 and the correlation coefficients which have been presented elsewhere. Another attempt to check the seeming disparity between the two sets of results we have made by employing a biserial $r$ which was computed to show the relationship between the dichotomous distributions of intelligence above and below the median. This computation has given us a value of $r$ of $-0.1418 \pm 0.145$. This sign of $r$ confirms the coefficients of correlation which we have presented and is, therefore, at variance with our graphic results in Figure 4. Our final attempt at explaining the discrepancy has consisted of dividing the subjects into quartiles in rankings in intelligence and by calculating the sum of the upper and lower quartiles and the sum of the divisions above and below the median, by employing the formula: deviation equals $(\Sigma X_1+X_2+X_3+X_4) - NM$, where $N$ equals the number of subjects in each group and $M$ equals initial error for all subjects. This computation has given us the following results:

| | |
|---|---|
| Upper 50% in intelligence | = + 43.76 |
| Lower 50% in intelligence | = — 97.24 |
| Highest quartile in intelligence | = — 62.12 |
| Lowest quartile in intelligence | = + 46.88 |

The values just presented indicate that the sum of the deviations of the upper two quartiles considered as a whole is greater than the sum of the deviations of the lower two quartiles, a result which accords with our Pearson product-moment coefficients, our biserial $r$, and our research of last year, to the effect that the more intelligent subjects are slightly superior to the less intelligent subjects in the Müller-Lyer test. It is the deviations, however, in the extreme quartiles, which in an ordinary coefficient of correlation have only a small effect in determining the amount and sign of the correlation, which here have been so large as to alter the relationship when it is presented in graphic fashion. The relationship between susceptibility to error in the illusion figure and the degree of intelligence is so low that one or two cases are sufficient to displace the correlation sufficiently so as to make the graphic picture in Figure 4 seem to conflict with the obtained $r$'s which we have presented.

The results which we have obtained in this investigation are

correspondingly similar to those obtained by Seashore,[3] he having reported a mean inaccuracy in his subjects of 21 per cent, while in our research the mean first trial error is 20.9 per cent. The mean error of the first five trials in our experiment amounts to 17.3 per cent, this being a close approach to the value of 15.5 per cent which we had obtained in our previous work. At the end of the practice, however, the amount of error had fallen in our experiment to 7.3 per cent as the mean error for the last five trials.

## Summary

We have found with our 33 subjects (1) no significant correlation between intellectual rating and susceptibility to error in the perception of the lines of the Müller-Lyer illusion figure, regardless of whether that error be measured as the initial error, the mean error of 30 daily trials, or the final error; (2) a very slight suggestion of a possible positive relationship between high intelligence and low error in the illusion; (3) definite practice effects over a period of 30 daily trials, involving an initial decrease of error less rapid than usual, followed by a gradually lessening amount of error from then on; (4) no significant correlation between intellectual rating and amount of improvement resulting from practice; (5) that initial ability is slightly and positively prognostic of final ability, such prognostication, however, being relatively hazardous; and (6) that those subjects of higher intelligence rating, as a whole, tend to overcorrect the illusion figure in the late stages of practice, while subjects of lower intelligence, as a group, tend to undercorrect the figure even after they have had much practice.

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## L'EXERCICE ET LA CAPACITÉ DE SE PERFECTIONNER DANS L'ILLUSION MÜLLER-LYER EN RAPPORT A L'INTELLIGENCE

(Résumé)

On a fait subir trente épreuves quotidiennes, dans des conditions d'illumination contrôlée et de distance contrôlée, à chacun de 33 sujets, l'épreuve consistant de la comparaison et du jugement des grandes lignes d'une forme Müller-Lyer. L'expérimentateur, à la dictée du sujet, a fait lui-même tous les ajustements nécessaires de la ligne mobile. Il a vérifié le degré d'erreur sur une échelle en millimètres cachée aux yeux du sujet. Le composé des données des erreurs a représenté, malgré les différences in-

[3]Seashore, C. E., *et al.* The effect of practice on normal illusions. (*Univ. Iowa Stud. Psychol.*, No. V.) *Psychol Rev., Monog. Suppl.*, 1908, **9**. No. 38, 103-148.

dividuelles, une assez juste approximation à la forme ordinaire de la courbe d'apprentissage—les sujets sont devenus progressivement moins susceptibles à l'illusion. Des résultats des tests d'intelligence, obtenus des examens avec les tests de l'American Council on Education, ont été corrélés avec les résultats des erreurs initiales, finales, et moyennes sur la forme de l'illusion. Presque nulle corrélation n'existe entre l'intelligence et la susceptibilité à l'illusion, bien que les sujets les plus intelligents soient un peu moins susceptibles que les moins intelligents. La capacité d'apprendre—de se perfectionner—a une corrélation de très petite valeur avec les évaluations intellectuelles. La capacité initiale, montrée dans les premières épreuves, prédit un peu une capacité finale dans l'expérience. Dans le dernier stade de l'exercice les sujets les plus intelligents tendent à corriger plus la portion à ajuster de la forme, tandis que le contraire est vrai pour les moins intelligents dont toutes les erreurs ont eu lieu parce qu'ils n'ont pas fait assez de corrections.

CROSLAND, TAYLOR, ET NEWSOM

## ÜBUNG UND DIE MÖGLICHE VERBESSERUNG BEI DER MÜLLER-LYER ILLUSION IM VERHÄLTNIS ZUR INTELLIGENZ

(Referat)

Dreissig tägliche Versuche im Vergleichen und Beurteilen der Hauptlinien einer Müller-Lyer Figur wurden mit 33 Vpn. bei kontrollirbarer Beleuchtung und Entfernung angestellt. Der Versuchsleiter machte selber, je nach dem Geheiss der Versuchsperson die nötigen Anpassungen oder Verschiebungen der beweglichen Linien. Den Irrtumsgrad ermittelte er durch ein Millimetermass, das jedoch den Augen der Vp. verborgen blieb. Die Summe der Irrtumsdaten zeigte—trotz individueller Unterschiede—eine entschiedene Ähnlichkeit mit der gewöhnlichen Form der Lernkurve; die Vpn. wurden nach und nach weniger von der Illusion beeinflusst. Intelligenzwertungen aus den Examen des "American Council" für "Education Tests," wurden mit den ersten, letzten und mittlern Irrtumswertungen der Illusionsfigur korreliert. Man fand fast keine Korrelation zwischen Intelligenz und Illusionsempfänglichkeit, obwohl die intelligenteren Vpn. etwas weniger von der Illusion beeinflusst werden, als die weniger Intelligenten, Lernfähigkeit, die Möglichkeit der Verbesserung, korreliert mit sehr geringer Wertung mit Intelligenzwertung. Angeborne Fähigkeit—wie die ersten Versuche beweisen—verheisst nur in geringem Masse eine endliche Fähigkeit in Bezug auf das Experiment. In den letzten Übungsstadien, neigen die Intelligenteren Vpn. dazu die beweglichen Linien übermässig zu verbessern, wogegen bei den weniger Intelligenten das Gegenteil der Fall ist, deren Irrtümer immer in Unterkorrektion bestehen.

CROSLAND, TAYLOR, UND NEWSOM

## ПРАКТИКА И УПРАЖНЯЕМОСТЬ ИЛЛЮЗИИ МЮЛЛЕР-ЛИЕРА И ИХ ОТНОШЕНИЕ К ИНТЕЛЛЕКТУ.

(Реферат).

Тридцать опытов ежедневно при известном расстоянии и определенном свете были проведены с 33 испытуемыми с сравнением линий в фигуре Мюллер-Лиера. Экспериментатор, по указанию испытуемого, сам вносил все необходимые коррективы в движущиеся линии. Он определял степень ошибочности посредством миллиметровой шкалы, скрытой от глаз испытуемого. Общая сумма ошибок, несмотря на индивидуальные отклонения, представляет собой достаточно близкое приближение к обычной кривой навыка—испытуемые постепенно становились менее восприимчивыми к иллюзии. Данные умственной одаренности, полученные после тестового испытания, были коррелированы с первоначальными, последними и средними ошибками при иллюзии с фигурами. Корреляцию между умственной одаренностью и восприимчивостью к иллюзии почти совершенно невозможно установить, хотя более одаренные испытуемые несколько меньше восприимчивы к иллюзии, чем менее одаренные. Данные упражняемости дают слишком низкую корреляцию с интеллектуальными данными. Способность, проявляемая вначале опыта, как видно из первых опытов, является прогностической в отношении результатов, полученных после упражнения. На последних стадиях опыта наиболее одаренные испытуемые стараются снова исправить уже исправленную оценку в то время как совершенно противоположная тенденция проявляется у менее одаренных испытуемых.

(Crosland, Taylor, and Newsom) Гросланд

# THE PHANTOM OF THE GESTALT*

*From the Department of Psychology of Bucknell University*

FREDERICK H. LUND

## A. PRESUPPOSITIONS OF *Gestalt-Theorie*

In their attack upon atomistic psychology and in their insistence that every experience is a Gestalt, there is no mistaking the intentions of the configuration psychologists. When, on the other hand, one begins to make more specific inquiries as to the nature and existence of the Gestalt one is likely to have difficulty. What, for instance, is its place and function in the experiential sequence? Is it to be located objectively in the stimulus situation, in the central processes, or in the response coordinations? And, again, does it exist prior to experience, or is it a product of experience? To any of these questions it is possible to find conflicting answers not only in current reviews of the subject but in the original literature. This ambiguity of doctrine is attributable chiefly to an apparent unwillingness on the part of its exponents to close in upon the Gestalt concept, possibly out of fear that it will vanish into thin air or turn out to be another name for certain well-known phenomena.

But the very intangibility of the concept is doubtless responsible for the sympathetic if not enthusiastic response received from the mystics, the vitalists, and those who feel that there *is,* or at least *ought* to be something the matter with certain mechanistic and naturalistic interpretations of today. Hence, the chorus of approval from the purposivists, the Bergsons, the McDougalls, and the Driesches. Indeed, with Koffka's insistence that "the universal Gestalt" is an original property of experience, that "meaning is *not* a property of experience,"[1] and Köhler's affirmation that "no experience is needed for a first formation of units in the visual field,"[2] we again feel ourselves in the atmosphere of the Hegelian Universals and the Platonic Forms.

*Received for publication by H. L. Hollingworth of the Editorial Board, January 23, 1928.

[1]Koffka, K. In Psychologies of 1925. Worcester, Mass.: Clark University Press, 1926. See pp. 138-143.

[2]Köhler, W. In Psychologies of 1925. Worcester, Mass.: Clark University Press, 1926. See pp. 174-175.

Now, we shall accept the contention of the configurationists that they are presenting an entirely new interpretation of experience, and agree with Koffka that "in 1912, Wertheimer stated for the first time the principles of *Gestalt-Theorie*."[3] But in so doing we are called upon to find something in their theory beyond the familiar fact that adult awareness is largely perceptual and conceptual rather than sensory (that is, awareness of objects rather than sensations), and, again, something more than the fact that the recurrence of certain stimulus combinations sets up a cortical disposition to react with the patterns and constructs thus formed whenever an element or portion of the stimulus situation recurs. These are well-worn descriptions and do not possess the novelty presupposed by *Gestalt-Theorie*.

With this in mind we shall analyze the leading contentions and preferred illustrations of this new brand of psychology and see if we can discover the feature of experience which better-known accounts are said to have overlooked.

### B. The Attack upon Psychological Atomism

The configurationists would first of all appear original in their attack upon structuralistic concepts. In fact, we are (by implication at least) led to believe that outside their own group all psychologists adhere to psychological atomism, either of the sensationalistic or behavioristic variety. However, it would be hard to find a psychologist today who accepts crass sensationalism (the doctrine that experience is a composite of sensational elements) or who would accept atomistic behaviorism (the theory that adult behavior is a compound of independent reflex mechanisms). As a group, certainly, American psychologists have been more inclined to follow William James in opposing the psychological chemistry of Wundt and his followers, and the structuralism by which all mental processes become compounds of sensational and imaginal elements. "No student of William James," to quote Calkins, "can have forgotten either the vigor with which he hews away, root and branch, at the current concept of sensations as 'composing' objects of consciousness."[4] And this author quotes the following

[3]Koffka, K. Perception: an introduction to the Gestalt-Theorie. *Psychol. Bull.*, 1922, **19**, 531.

[4]Calkins, M. W. Critical comments on Gestalt-Theorie. *Psychol. Rev.*, 1926, **33**, 155.

statement from James in support of her statement: "A pure sensation is an abstraction . . .Any quality of a thing which affects our sense organs does also more than that; it arouses processes in the hemispheres, the results of which in consciousness are commonly described as ideas which the sensations suggest . . .All brain processes are such as give rise to what may be called Figured Consciousness. If paths are irradiated at all they are irradiated in consistent systems and occasion thoughts of definite objects, not mere hodge-podges of elements. Brain paths. . . .once excited together, thereafter tend to vibrate as a systematic whole."[5] This denial of sensationalism is more representative of the attitude of American psychologists and is obviously not original with the Gestalt psychologists.

Nor would it be fair to impute the behaviorists with the statement that all behavior results from a chance combination or hodge-podge of isolated reflex elements, even though they do contend that acquired adaptations are built up from native reaction units—which is really very different.

As a matter of fact, the Gestalt psychologists in declaring that experience is not a composite of sensational or behavior units but a composite of "wholes," "units and sub-units," called *Gestalten* would appear to have substituted one form of atomism for another. However, to understand human experience we must have clearly before us *not* a set of *Gestalten,* nor a complex of sensations and images, nor a system of reflex patterns—conditioned or unconditioned —but a living organism adapting itself to a complex and changing environment, an *organic* whole with a great many interrelated functions articulated and inseparable except under artificial analysis.

### C. The Concept of "Wholes" and "Parts"

In the ardor of their attack upon extreme atomism the configurationists come dangerously near asserting that "every experience is a *Gestalt*" (Calkin's interpretation)[6]—a "whole" or "structure" which is "not open to analytic observation."[7] Analysis is banned on the ground that it necessarily leads to the discovery of pseudo-psychic atoms. "The configurationists," to quote Helson, "are con-

[5] *Ibid.,* p. 157.
[6] *Ibid.,* p. 135.
[7] Koffka K. Introspection and the method of psychology. *Brit. J. Psychol.,* 1924, **15**, 161.

vinced that the ultimate sensational and behavior units are a product of ratiocination and reflection or of an artificial 'laboratory attitude' toward the facts. Objects, meanings, values and dynamic processes are thus analyzed into meaningless units which have nothing to do with the patterns in which they originally appeared."[8]

We must take experience, then, as a series of *Gestalten,* or of unanalyzable "wholes." These *alone* comprise the stream of events. But how can we have "wholes" without "parts," unities without elements, experiences in which sensory and reaction elements have, at best, pseudo-existence? If I have witnessed a street accident and endeavor to give an accurate account of the experience, is my recall of the automobiles, the people, and the place *real,* my recall of the color and brightness of the objects *unreal?* Is my recall of an individual running to the rescue real, my recall of a peculiarity in his gait or some significant movement unreal? Is my consciousness of the road, as such, real, my consciousness of its roughness unreal? But what, may we ask, are the "wholes" within this experience, and what are the unreal "parts" or "elements"? In later retrospect my memory of the experiences of that day may include "the accident" considered as a unit possibly. But this is surely not the Gestalt or "whole" which will be distorted if I begin to recount its different features, for only in terms of the latter is it intelligible. And yet, is it not as much a "whole" as anything? Possibly the "wholes" are the people, trees, cars, crossroads, etc. Still, a mere account of the existence of these objects would hardly give my auditors much of an idea as to what happened. Are "wholes" then of a more functional nature—the movement of the cars resulting in a collision, the act of saving a person from under the débris? An account presented in such terms is doubtless more to the point. But whatever our decision in this respect it can hardly be said that one ever reaches a stage in the analysis and description of the events beyond which more minute details renders "the picture" less intelligible. The act of rescuing increases in vividness with the details given, even to the particular series of movements by which it was accomplished; and the individual making the rescue gains individuality as we describe his appearance, even to the color of his hair and the pallor of his features. In short, "wholes" presuppose parts and are intelligible only in terms of parts. And what shall con-

[8]Helson, H. The psychology of Gestalt. *Amer. J. Psychol.,* 1925, **36**, 345.

stitute a whole or what shall constitute a part depends upon our point of view and interest at the time. One may react to an automobile "as a whole" in the event that one is interested in it as an automobile; or one may react to it "as a part" in the event that it forms a significant feature in a scene. The same would be true of one of its wheels, or of a spoke within one of the wheels. Indeed, what is there to prevent the color from being the object of curiosity and the thing reacted to "as a whole" quite apart from anything else? It is what we react to that is remembered and that constitutes experience. As such, it may be quite varied in nature with components chiefly sensory, formal, structural, or functional.

In view of the above analysis the typical Gestalt assertion that "the whole determines the nature of the parts" and the still stronger statement of Ehrenfels that "the configuration possesses properties over and above its parts and not derivable from them"[9] become highly inadequate if not meaningless. Awareness in connection with a situation such as the one we have described is not made up of a procession of "wholes" or *Gestalten* but of a variety of interdependent events with sensory, formal, functional, and sequential relationship. And while it is true that the coloring given to some particular element of a situtation is influenced by its setting, it is any less real. No aspect of experience remain uninfluenced by the characteristics of the parts composing it.

## Sensations, Feelings, and Emotions

We cannot agree then that sensations are not components of the experiential sequence. That they do not appear in pure or unmodified form is nothing peculiar to them and does not make them any less real. No aspect of experience remains uninfluenced by the aspects with which it is associated. Formal features influence the character of the sensory and vice versa. It is no more true that the character of the constructs influences the character of the parts than the reverse. It is only in the abstract that we can consider the constructs immune to the modifying influence of component features, and to make purity or immunity from such influences a prerequisite of reality is to make all experience unreal.

The feelings and emotions have been almost if not entirely ignored by the configurationists. This neglect apparently results from an

[9]Cited by Helson, *ibid.*, p. 360.

inability to place them in the preferred categories: they cannot very readily be fitted into the configured orders, nor can they very well be denied phenomenological existence with the sensations. Yet the feelings and emotions, like sensations, while not assuming as prominent a rôle in experience as the more formal features, cannot therefore be disregarded. In fact, they are not always so unimportant. In my recall of the particular accident we have been considering, the things which stand out most vividly are of a sensory and emotional nature. That surge of feeling and excitement, the quiver of limbs and frame accompanied by certain unfamiliar sensations, are still the first to come to mind with my recall of the event. And while I am unable now, or even then, to give an account of the figure and person of a man engaged in the rescue of a mutilated form, I still see mirrored his pale, drawn features. With even greater vividness there persists an image of "a ghastly pool of steaming crimson," the consciousness of which originally blotted out all else for a moment. That pallor, that crimson, the feelings and emotions, were surely as real as any "forms" or configurations to which I might refer.

### E. Objectification of the Gestalt

But the *Gestalten* themselves, *what* and *where* are they, and how do they originate? On the whole, Gestalt literature favors identifying them with the objective stimulus situation. To make the configuration a property of the reaction mechanisms might too obviously identify it with the familiar perceptual or conceptual reaction which the sensory elements tend to touch off. As a property of the stimulus situation, some quotations from Köhler are perhaps most to the point: "Are there objective units in a field of vision? Yes, there are and we have been considering them for some time. It is not arbitrary and abstract thinking that makes these groups or spots or rectangles or things in my visual field. I find them there as optical realities not less real than their color." "Intimately related to the experience of real units and boundaries in the field of vision we find the fact that there are *forms* in this field . . . .Some concrete forms *are simply there* in vision, not less than colors and brightness. . . .There is no field of vision which you have in everyday life in which you might not find thousands of geometrical patterns of all varieties."[10]

[10] *Op. cit.*, pp. 176-181.

This runs counter to the usual rendering by which the material given in sense is ordered or arranged no more one way than another and that object-consciousness or the tendency to organize the sensory elements to form concrete units is a central process and depends upon experience. For this a great deal of evidence is available in discussions and experiments on developmental features of perception, illusions, and ambiguous perspective. Adult subjects, when viewing irregular outlines, as in the experiment with inkblots, or when working with other meaningless designs, tend to see in the sensory elements the contours of objects familiar to them. The same line may be seen either as the boundary of an "object" on its left or as the boundary of an "object" on its right. Which "object" shall be seen most readily is chiefly a matter of familiarity with the "object." That the various demarcations of color and brightness in the visual field are seen as the outlines of objects, and correctly attributed with respect to these objects or forms, is not because "these forms *are simply there,*" as Koffka says, but because experience has taught us to see them in this way. In other words, there is no configured environment. What we experience as wholes or unities are such only because of *frequent togetherness-in-experience of certain stimulus combinations.*

## F. Origin of the Gestalt

But it is this last statement which the confiurationists concur in denying, since they do not believe the Gestalt is a product of experience. To quote Koffka: "We find at the beginning . . . unitary, articulate, meaningful wholes; to which we apply the name *Gestalt,* configuration, or structure. Development starts not with chaos nor with a multitude of mental elements, without order or meaning, but with structures, however primitive their character may be."[11]

According to this statement, our first mental reactions are configurational. Thus, in addition to the objective configured environment, considered in the previous section, we are now told that our earliest mental reactions are strictly formal in their composition. This presupposes original configurational processes in the nervous system. For if constructs on the reaction side are not acquired products built up out of original reaction units, they must

[11]Koffka, K. In Psychologies of 1925. Worcester, Mass.: Clark University Press, 1926. See p. 141.

be native to the physiological substratum. Indeed, Hartmann tells us that "the main thesis of the configuration theory may be said to be that physical configurations possess properties parallel to their phenomenal correlates."[12] And, as Helson puts it, "If the wholes, patterns, formal aspects of experience *cannot* be built up out of summative elements, then it becomes necessary to postulate configurational processes in the nervous system"[13] which, not being acquired products, must be present from the beginning. In other words, the newborn child not only responds to a configured environment but responds by way of ready-made configured response mechanisms. In its first experience of a dog there must be represented not only a configured environmental "dog" but a ready-made perceptual response "dog."

G. Learning as a Product of Deliberation and Insight

To be consistent with these "phenomenological" and physiological conceptions, the configurationists have been forced to adopt the attitude that learning is not gradual but sudden, that the apprehension of meaning occurs through sudden insight, and the mastery of a situation does not occur through "trial and error adjustments" with gradual elimination of unnecessary responses. The following statements from Koffka and Köhler are typical: "Chimpanzees solved their problems *not* through trial and error, the correct solution was slowly and painfully selected out of a number of inexpedient movements, the correct activity began abruptly from a stage of deliberation."[14] "He (the orang-utan) had solved the problem in one lucky moment, his curve of learning showing an abrupt descent."[15]

The question is not whether sudden drops in the learning curve may occur as the result of insight, but whether such learning can possibly be considered typical. Even in Köhler's report of the apes' learning to stack the boxes there is evidence of gradual perfection of appropriate adjustments following behavior which may best be described as "trial and error." When the apes were let into the room in which the bananas were suspended from the ceiling but out of reach they apparently did not assume a thoughtful and

[12]Hartmann, L. Neue Verschmelzungsprobleme. *Psychol. Forsch.*, 1923, **3**, 391.

[13]*Op. cit.*, p. 515.

[14]Koffka, K. In Psychologies of 1925. Worcester, Mass.: Clark University Press, 1926. See p. 135.

[15]Köhler, *op. cit.*, p. 149.

deliberative attitude until with sudden insight the key to the situation was discovered, the boxes brought from the corner, and methodically stacked below the fruit. On the contrary, there appears to have been much agitated and unnecessary running around, and much "trial and error" jumping at the fruit. Such behavior, however ineffective, is very natural in the situation, and very similar to the "random" behavior reported in the maze and "puzzle-box" experiments. The animal reponds with such movements as are already within its repertoire of adjustments, with those which the stimulus situation most naturally elicits, suggests, or redintegrates. And as for the manipulation of the boxes it should first of all be observed that in most cases it was necessary first to allow the apes to observe their use. And even in the appropriate attempts to stack them, the act cannot invariably have been accompanied by insight as to the purpose of the operation. On the other hand, as seen from the motion pictures portraying their behavior, they had merely come to associate the boxes and their stacking with the total situation. Their behavior was "random" and "trial and error" not only in the fact that even the most intelligent would experience considerable difficulty in learning to stack the boxes but that the boxes were frequently pushed in the wrong direction when taken from the corner, and manipulated without apparent apprehension as to the object of their behavior. In fact, one "stupid" ape instead of bringing the boxes out on the floor below the bananas gave them some erratic jerks, climbed upon them, and made an ineffectual dash for the bananas.

It is true that the configurationists have been able to find in this and other learning situations features of behavior which give evidence of apparent insight, but it is more than likely that these features represent elements of transfer from previous, learned adjustments, and were originally perfected from just such random reactions as those with which they are now interspersed.

It should also be observed that the preferred illustrations of "sudden insight" are of such a nature that the right adaptations either occur or do not occur. In no case do they choose situations in which a complex series of adjustments has to be built up. In fact they do not find the usual learning situation resulting typically in a learning curve with gradual descent at all apropos. Koffka states that "the puzzle-box (and maze) and the learning of nonsense syllables represent highly artificial conditions. In both cases the

object of experimentation, the animal or the human observer, is confronted with a situation completely meaningless."[16] They seem to forget that unless such situations are employed we cannot observe the learning process from the beginning. A thing is meaningful only because it rearouses associations and adjustments previously formed. That which now gives meaning must once have been as meaningless as nonsense syllables. And it is doubtful whether an illustration of "sudden insight" can be discovered which does not depend on previous learning.

### H. Criticism of Associationism

In turning now to the Gestalt theorists' criticism of the atomistic concepts of the older associationists we find nothing new and nothing which cannot be explained without the aid of the mysterious Gestalt. Koffka restates associationism as follows: "If A, B, C, . . .once, or several times, have been present in experience as members of a configuration, and if one of them appears bearing the membership character, then the tendency is for the whole to be completed, more or less fully and vividly and completely."[17] What Koffka apparently has in mind is more accurately stated in Hollingworth's familiar principle of redintegration, known to us long before we had heard anything about Gestalt: The recurrence of a signficant element from a past stimulus situation tends to reinstate or to *redintegrate* the entire response sequence with which it was associated. This is because of the synergical relation which comes to obtain between the co-stimuli and the co-responses. Thus, John's hat reminds us of John or has redintegrative efficacy relative to John and the associations which cluster around him because it has frequently appeared as a co-stimulus with John.

Apparently, what Koffka calls *Gestaltdisposition*[18] (interpreted by Calkins as "the tendency to reinstate in imagination a configuration already apprehended") is again another expression for redintegration.

### I. Concept of Meaning

Meaning, we are assured, as an aspect of the Gestalt is no more

[16]Koffka, K. In Psychologies of 1925. Worcester, Mass.: Clark University Press, 1926. See p. 134.

[17]Koffka, K. Die Grundlagen der psychischen Entwicklung. 1921. See p. 176.

[18]Koffka, K. Psychologie. In Dessoir's Lehrbuch der Philosophie. Berlin: Ullstein, 1925. See Vol. II, pp. 537f., 540, 571f.

empirically derived than the Gestalt itself. "Experience," says Koffka, "does not create meaning, but *transforms* meaning; which implies that meaning itself is not a product of experience—mental development does not lead from the meaningless to the meaningful, but from one kind of meaning to another kind of meaning."[19] Now, to say that meaning is not a product of experience and that all experience can do is to transform meaning is to say that the meaningless cannot acquire meaning since there is no meaning in this case to transform. However, in the learning of a new language are meaningless sensory elements not acquiring meaning? The infant surely does not start with the entire set of meanings which make up the world of the adult! As its experience increases, its meaningful reactions increase. Sensory elements come to *mean* or function for other familiar objects or situations through their redintegrative efficacy.

For the configurationists "the mystery of how things come to mean" seems to be sufficient justification for invoking the Gestalt concept. For, as they would have it, in so far as the meaningless cannot come to mean, meaning must exist prior to experience as an inherent aspect of the Gestalt. But there is hardly any special occasion to marvel about meaning. It is a matter of everyday experience to notice how associated sensory elements tend to lead over into the same reactions, gain the same ulterior reference—that is, come to *mean* those obejects with which they are associated. Words, symbols, or signs, originally meaningless, come to function for the objects with which they have been present as co-stimuli.

### J. Ambiguous Perspective

The configurationists have correctly observed that ambiguous perspective and puzzle-pictures show clearly that object-awareness cannot be considered a necessary product of the elements present to sense, since in this case the same sensory elements result in alternating constructs. Here they are required to give up the idea of objective configurations and to identify them with the central processes—in this case, the perceptual reactions. But the singularity of the perceptual reactions in ambiguous perspective does not call for any special Gestalt rendering. They furnish the best illustration of Woodworth's definition of the perceptual response as a secondary

[19]Koffka, K. In Psychologies of 1925. Worcester, Mass.: Clark University Press, 1926. See p. 138.

response to sensory stimulation. In this case, as in aphasia, it is possible to distinguish between the sensory and the perceptual response. Where two perceptual reactions to a given situation are possible, it has been found that one of these may be facilitated and even favored to the exclusion of the other, as the result of habitutation, and of attention's being given to those sensory elements which, because of more intimate association, have greater redintegrative value relative to the response in question.

### K. Experiment on "the Grays"

The favorite illustration of the Gestalt phenomenon is an experiment in which an animal is taught to pick its food from the lighter of two containers. When a third container lighter than the one to which the animal has been reacting is introduced, the animal reacts to the lighter of the two now present. This is said to demonstrate a configured response. To the writer it demonstrates the greater redintegrative value of one aspect of the stimulus situation than another. It is the case of reacting in the direction of increased stimulation and may be observed in the lowest forms of marine life when the animal is taught to seek its food in the lightest part of the aquarium. It may also be taught to seek its food in the darkest part. The animal does not necessarily seek any *particular* degree of light intensity, but moves around till it finds the place with greatest or least amount of light stimulation. It is not reacting to a Gestalt but to increase or decrease in intensity which is one of the simplest aspects of the stimulus. Of the same nature would be the case of reacting to the "lighter than" or the "darker than" of two gray surfaces.

That organisms should be more disposed to react to form or outline of an object than to its color or brightness is due to the greater constancy and greater distinctness of the former sensory elements. It is a rather familiar fact that we tend to ignore those elements in a stimulus situation which vary or are less distinct. And it is fairly safe to assume that Köhler would have secured different results if the difference in the intensity of the grays had been sufficiently increased, since under these conditions the particular strength of this stimulus element would have gained greater redintegrative potency.

## L. Transposability of the Gestalt

Köhler, following Ehrenfels, finds that the *Gestalten* are transposable (*transponierbar*), that "they remain intact in their specific qualities when the absolute data (*Gegebenheiten*) are displaced in a definite fashion."[20] Helson likewise in his interpretation of this phase of the Gestalt tells us that only similar and not identical elements need be present in order that recognition of the Gestalt shall take place.[21] Here again it would seem that the configurationists are merely calling attention to the part-whole relation which frequently obtains within the stimulus-response sequence. All that we can ever say of the stimuli or the *Gegebenheiten* is that they are similar, not identical, and that the stimulus elements previously arousing awareness of a given object are only partially and never completely present. That the object-awareness "train" may be aroused by sensory elements only similar in their composition, and that the same object-awareness may result from very different sensory elements (the shrill whistle of the locomotive, or the grind and clatter of the wheels may arouse the same perceptual response) is not indicative of "transposability of *Gestalten*," but of the fact that the same perceptual reaction may be aroused by quite varied stimuli, provided these stimuli or similar ones have been associated with this response before. There is never absolute sameness or completeness about the sensory elements, but merely similarity and partial identity. Nowhere is this clearer than in art. In a painting one has neither the "true" sensory elements, nor does one have symbols exactly. The colors, shades, and contours possess their particular redintegrative value because they reproduce in partial form the arrangement of sensory elements belonging to the situation represented in the picture.

## M. Movement

How consciousness of movement can arise from a series of stationary phases of a moving object, as in the cinematograph, was the initial puzzle of the configurationists. To explain the riddle the Gestalt concept was resorted to, and elaborate experiments on objects assuming successively different space positions were undertaken to determine the particular combination responsible for the

[20]Köhler, W. Die physischen Gestalten in Ruhe und im stationären Zustand. Erlangen: Weltkreisverlag, 1920. See p. 17.

[21]*Op. cit.*, 1926, **37**, 47.

Gestalt. However, it is doubtful whether movement is ever *given* objectively, and whether the stimuli ever do more than *suggest* movement. And what is more, we do not need successive space positions of objects to carry this suggestion effectively. A *single* stationary phase of an object, if it represents a phase frequently apprehended visually and thus intimately associated with the action, will be sufficient to *suggest* or arouse consciousness of that action. Hollingworth's experiments on "the law of the resting point" have clearly shown this. An individual performing a given activity goes through a series of adjustments (each of which represents motion which cannot be "seen," only inferred), assumes a series of positions, some of which become more intimately associated with the activity because they represent momentary rest periods and thus result in clearer images. The latter will thus gain redintegrative value even when reproduced singly. In so far as the cinematograph reproduces these sensory elements more completely, movement becomes more "real." The awareness of movement itself, or the perceptual response, is made up of all the associative responses formed in connection with the activity and may be rearoused more or less completely, or even left in its nuclear state as when our awareness does not far exceed the symbolic language expression.

## Summary

The Gestalt, with the rendering it has been given by its leading exponents, has no assignable value in psychological description nor any real existence within the experimential sequence, because:

1) It is assumed to be of non-empirical origin, existing with the first experience and not a product of experience, and hence, of the same abstract and phantom-like nature attributable to the Hegelian Universals and Platonic Forms.

2) It is considered immune to the modifying influences of sensational elements and changing circumstances of experience.

3) It cannot be identified with the stimulus aspects of experience because these constitute too changing and fluctuating a material.

4) Nor can it be identified with the response coordinations, perceptual or overt, since the ordered and structural features of these are well known and do not possess the novelty attributed to the Gestalt.

Having taken the position that the Gestalt is not empirically derived, not dependent upon sensational elements, nor built up gra-

dually from primitive reaction units, the configurationists have been forced into the following assumptions:

1) That analysis of the nature and origin of structural features of experience necessarily leads to distortions of experience and the discovery of pseudo-psychic atoms.

2) That learning does not proceed by "trial and error" nor by the gradual selection of correct adjustments from original random behavior.

3) That the acquistion of a complex mental or physical co-ordination is not a gradual process but typically occurs as the result of sudden insight.

4) That meaning is not a product of experience and that all experience can do is to transform meaning.

To say that meaning as a function of symbols and sensory elements is not acquired and that all experience can do is to *transform* meanings, is to say that the infant must start with a set of meanings equally extensive with those of the adult, which is most unwarranted. And to say that the constructs of experience, perceptual or overt, are not built up gradually from original reaction units is to suppose that physiological constructs, corresponding to the phenomenological, also have existence from the very beginning, which is equally untenable.

That the formal or linear features of the stimulus situation acquire greater significance than coloring or brightness does not make the latter any less real, nor does it permit us to say that the former are independent of, or unmodified by the latter. Structural or linear features, as compared with the qualitative, attain greater redintegrative value in the stimulus-response sequence because of their greater distinctness and constancy. And what appears in adult experience as *objective* unities or "wholes" can be attributed only to the frequent togetherness-in-experience of certain stimulus combinations.

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## LE FANTÔME DE LA GESTALT

(Résumé)

La Gestalt n'a aucune valeur assignable dans la description psychologique ni existence réelle dans la séquence des expériences, (1) parce qu'on suppose qu'elle est d'origine non empirique, existant avec la première expérience et non comme produit de l'experience, et par conséquent de la même nature que les Notions Universelles de Hegel et les Formes Platoniques; (2) parce qu'on la considère réfractaire aux influences modifiantes des éléments sensationnels et des circonstances changeantes de l'expérience; (3) parce qu'on ne peut l'identifier avec les aspects de l'expérience à l'égard des stimuli parce que ceux-ci constituent une matière trop variable; (4) parce qu'on ne peut l'identifier avec les coordinations des réponses, perceptives ou ouvertes, puisque les traits d'ordre et de structure de celles-ci ne possèdent pas la nouveauté attribuée à la Gestalt.

Ayant adopté la vue que la Gestalt n'est pas empiriquement dérivée, les adhérents de la configuration ont été forcés dans la contention que l'acquisition de toutes les fonctions en rapport ou identiques avec *Gestalten* a lieu au moyen d'éclaircissements soudains et d'avancements spontanés aux niveaux plus hauts. C'est à supposer que l'enfant en est doué à sa naissance d'une façon quelconque et que le stimulus approprié est tout ce qu'il faut pour les faire naître. Mais les perceptions, les significations, et les actions habiles ne naissent spontanément ni n'apparaissent comme des "élaircissements soudains" mais sont élaborées peu à peu des procédés élémentaires pour former les unités perceptives de la vie mentale et les coordinations complexes du comportement ouvert. La fréquente réunion de certaines combinaisons de stimuli dans l'expérience détermine l'unité perçue des constructions de la vie perceptive; un choix heureux et la combinaison des unités données de réaction déterminent les constructions du comportement ouvert.

LUND

## DAS PHANTOM DER GESTALT

(Referat)

Die Gestalt hat keinen nennbaren Wert im psychologischen Begriff, noch ein wirkliches Dasein in der Erfahrungsfolge (Sequenz) da sie (1) keinen empirischen Ursprung hat indem sie schon bei der ersten Erfahrung vorhanden ist, und zwar nicht als Erfahreungsprodukt sondern ähnlich den Hegelschen Allgemeinheiten und den Platonischen Formen; (2) Weil man sie für inmun gegen den mässigenden Einfluss der aufregenden Elemente und der veränderlichen Umstände der Erfahrung hält; (3) Weil man sie nicht mit Stimulusaspekten der Erfahrung identifizieren kann, da diese zu veränderlich sind; (4) noch kann man sie mit den Koordinations reaktionen identifizieren—wahrnehmbar oder offenbar—da die geordneten und die inneren Merkmale derselben nicht die Neuheit besitzen welche man der Gestalt beilegt.

Als Vertreter der Ansicht, dass die Gestalt nicht auf empirische Weise entsteht, waren die Konfigurationisten zu der Behauptung gezwungen, dass alle Funktionen die sich auf *Gestalten* beziehen oder damit identisch sind, durch plötzliche Erkenntnis oder ganz von selbst entstehen und unwillkürlich ein höheres Niveau erreichen. Das heisst, man nimmt an, dass dem Kind diese Anlagen angeboren sind und es nur der rechten Anregung bedarf dieselben zu wecken. Doch Wahrnehmungen, Bedeutungen und Geschicklichkeit entstehen nicht ganz von selbst, noch erscheinen sie als "plötzliche Erkenntnis," sondern sie entwickeln sich allmälig aus elementaren Vorg-

ängen zu dem Wahrnehmungsvermögen des Geisteslebens und der komplizierten Koordinationen des äussern Benehmens. Häufiges Zusammentreffen oder Übereinstimmen der Erfahrung gewisser Stimuluskombinationen bestimmen die empfundene Einheit der Faktoren (constructs) des Wahrnehmungsvermögens; richtige Auswahl und Verbindung der vorhandenen Reaktionseinheiten bedingen die Faktoren des äussern Verhaltens.

LUND

## ИЛЛЮЗОРНОСТЬ „*GESTALT*".

(Реферат).

Понятие „*Gestalt*" не имеет специальной ценности для психологического описания и реально в опыте не существует, так как предполагается, что „*Gestalt*" не эмпирического происхождения, что он существует уже с самого начала опыта, а не в результате его и поэтому он носит такой же характер, как Гегелевская всеобщность и Платоновские формы; его считают свободным от модифицирующего влияния чувственных элементов и изменчивых условий опыта; его нельзя отождествлять с раздражителями, данными в опыте, так как они представляют собой слишком неустойчивый материал, его также нельзя отождествлять с реакциями, так как структурные черты последних не обладают новизной, приписываемой „*Gestalt*".

Становясь на точку зрения не эмпирического возникновения „*Gestalt*", сторонники последнего вынуждены придти к выводу, что приобретение всех функций, имеющих отношение к „*Gestalt*", или сходных с ним, происходит через внезапную интуицию, или благодаря спонтанному переходу к более высокому уровню познания. Это предполагает, что ребенок наделен этими качествами с детства и требуется только надлежащий стимул, чтобы вызывать их к жизни. Но восприятия, значение, разумные акты не возникают спонтанно и не появляются интуитивно, но вырабатываются постепенно из элементарных процессов, образующих единства психологических восприятий и сложную координированность внешнего поведения. Частое появление в опыте некоторых комбинаций раздражителей определяет конструкцию восприятий; успешный выбор и комбинирование данной системы реакций определяет наше поведение.

**Лунд (Lund).**

# IMAGERY, CONATION, AND CEREBRAL CONDUCTANCE*

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## 1. INTRODUCTION

The object of this research has been to make a mathematical description of the time changes of the intensity of imagery, including the ups and downs, the refusals, delays, sudden onsets, fadings, recoveries, and disappearances of the intensity of relevant imagery, which comes, or fails to come, in response to trying-to-think with various intensities.

It is a problem more in induction than in deduction.

If the description which is finally attained should accord with modern physiology, so much the better; if not, it still may be true psychology.

"Trying-to-think" is an active variety of attention. The outward signs are to be seen at examinations, and include some or all of: tense facial muscles, slight frown, eyes fixed and half closed, restless finger movements. We get the experience whenever we try to solve a puzzle of any kind. "Imagery" is used in its ordinary psychological sense.

No progress can be made in the desired direction unless it is possible to estimate numerically the intensities of imagery and of trying-to-think, purely by introspection. Fechner[1] asserted the possibility of thus estimating sensations. But he made other indefensible assumptions. W. James[2] and C. S. Myers[3] have contradicted him. G. F. Stout[4] saw no objection to sensations being measured, but added that it had not yet been done. The question has been dis-

*Received for publication by William McDougall of the Editorial Board, May 9, 1928.

[1]Fechner, G. T. Psychophysik. Leipzig: Breitkopf, 1860.

[2]James, W. Principles of psychology. New York: Holt, 1901. See Vol. I, p. 547.

[3]Myers, C. S. A text-book of experimental psychology. Cambridge: Cambridge Univ. Press; New York: Longmans, Green, 1911, 1925. See p. 252.

[4]Stout, G. F. A manual of psychology. London: Clive, Univ. Tutorial Press, 1915. See p. 307.

cussed at a Symposium.[5] For present purposes the best evidence that intensity of sensations can be numerically estimated is contained in a paper called "Loudness and Telephone Current" by the present writer and J. S. Ross.[6]

Once numbers have been assigned to introspective intensities, an algebraic treatment of those numbers becomes possible. And if we ever slip into loose but convenient phrases like "the square of the intensity of imagery" or "the logarithm of the intensity of trying," it must be understood that the correct though cumbrous form would refer instead to the square or logarithm of the number attached as a label to the intensity.

Let then the *intensity of trying to think* be represented by a number which we denote by $v$.

Let the *intensity of relevant imagery* be measured by a number symbolized by $m$.

Let us begin by studying $m$ in relation to $v$ and to time $t$.

## 2. Experiments

### 2*a*) *Consistency*

It would, of course, be desirable to survey mental types. But in a preliminary study like this it is more important that all the observations should fit together into one set of "brain constants"; therefore they have all been made on a single subject, namely, the writer.

It is less difficult to estimate an intensity than to estimate the uncertainty of that estimate; and in this paper there are no estimates of uncertainty. But for sensations, on the advice of Mr. F. C. Bartlett, some estimates of uncertainty will be found in the paper on "Loudness and Telephone Current."[6]

### 2*b*) *Persistence, Success, and Its After-Effects*

*Experiment 1A.* A chronograph[7] having three pens writing on a moving band of paper was arranged so that a standard clock marked seconds by one pen. The subject in another quiet room pressed a

[5]Myers, C. S.; Hicks, G. D.; Watt, H. J.; & Brown, W. Are the intensity differences of sensation quantitative? *Brit. J. Psychol.*, 1913-14, **6**; I, 137-154; II, 155-174; III, 175-183; IV, 184-189.

[6]To be published in this journal. See other supporting evidence in the *Brit. J. Psychol.*, 1928 or 1929, under the titles, "Thresholds When Sensation is Regarded as Quantitative" and "Quantitative Mental Estimates of Light and Colour" by L. F. Richardson, also a further work by R. S. Maxwell.

[7]Cambridge Sci. Instr. Co., List No. 88, Instrument 8854 is similar.

key with his right hand whenever he was trying to think and pressed another key with his left hand whenever he had relevant imagery. These keys operated electrically the other two pens. A dictionary or index was opened at random. Looking at the top word, the subject tried to think of its opposite. As soon as he had succeeded, however imperfectly, he looked at a different word and tried for its opposite. Several such distractors were sometimes inserted. Finally, he tried again to think of the opposite of the first word.

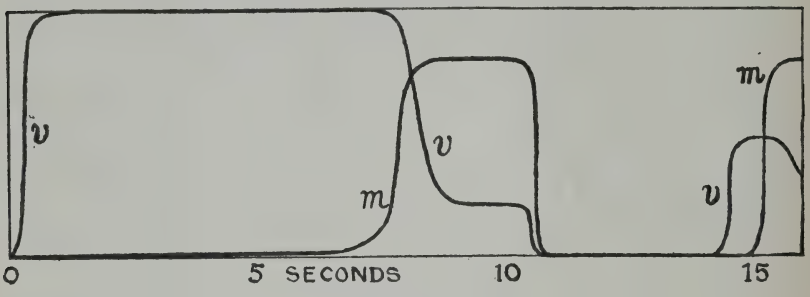


FIGURE 1
(See qualifications in 2*d*)

Table 1 shows that the "opposite," previously thought of, comes up much more rapidly at the second trial, especially if the interval between the two trials is not more than 15 seconds.

TABLE 1

| First stimulus word | Time of trying in secs. | Opposite thought of | Beginning of renewed trying on original stimulus seconds from start | Duration of renewed trying before idea revived seconds | Number of uninvited recurrences |
|---|---|---|---|---|---|
| censorship | 2.8 | liberty | 7.3 | 0.1 | 1 |
| dramatic rights | 14.1 | no dramatic rights | 23.5 | 0.4 | 1 |
| half profits | 3.1 | whole profits | 11.6 | 0.5 | 0 |
| imposition | 9.1 | half-holiday | 17.0 | 0.9 | 0 |
| misprints | 2.0 | correctness | 10.4 | 0.6 | 0 |
| shoulderhead | 5.0 | tailpiece | 13.8 | 0.7 | 0 |
| helpless | 2.5 | competent | 20.1 | 1.6 | 0 |
| counterplot | 9.6 | friendly treatment | 42.0 | 3.8 | 0 |
| orange | 3.1 | lemon (silly) | 34.5 | 2.1 | 1 |

Figure 1 shows the intensities given by introspection for a typical experiment. We have to find equations for these curves.

### 2c) *The Imageless Suspense*

A referee who read an earlier version of this paper objected that "the author wrongly supposes that what is thought of must necessarily occur in the form of imagery." This objection deserves a careful answer. Now the relation of imagery to thought has been reviewed by Spearman.[8] After re-reading these chapters I reply that the subject of the present experiments ordinarily thinks with inner speech. Whether by means of it, or merely to the accompaniment of it, is not easy to say. Visual images also occur and are used, but they are dim, inaccurate, and sometimes difficult to evoke. Tactile images can be vivid, but are seldom used. Images of any sort are usually present "on business." Occasional images intruding irrelevantly are neglected and usually die of neglect. But the decision as to what is relevant seems often to precede by a fraction of a second the inner word expressing the decision; so that, during that short time, thought is either imageless, or its image is unnoticed, or it might be said that the thought is unconscious. This agrees with T. V. Moore's statement that ". . . . there is a distinct interval of time between the perception of the meaning of a word or picture and any other subsequent imagery."[9] In solving problems as in Experiment 1 this imageless suspense may lengthen out to several seconds, as in the early part of Figure 1, which has been likened in the physiological note (2*n*) to the summation period. But extensive trains of imageless thought do not occur in me. Unlike the subjects of Martin,[10] if I try to suppress all images altogether, thought dwindles and I drift towards sleep.

This describes age 46; at age 15 visual images were numerous, bright, and easy to evoke, while appropriate words were difficult to find.

Another well-known psychologist has objected that the phrase "trying-to-think" should be replaced by "trying to evoke images." The proposed alteration seems to me unsuitable. In the experi-

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[8]Spearman, C. E. The nature of intelligence and the principles of cognition. New York: Macmillan, 1923. See chaps. XII and XIII.

[9]Moore, T. V. Image and meaning in memory and perception. (Psychol. Stud. Cath. Univ. Amer.) *Psychol. Monog.*, 1919, **27**, No. 119. Pp. 296.

[10]Spearman, *op. cit.*, p. 190.

ments, I tried to think in my habitual way, and as a consequence images happened.

But it must be admitted that the great variety in the use or disuse of imagery by different persons[11] does restrict the application of the present experiments.

*Experiment 1B. To observe what, if anything, happened during the imageless suspense,* the following routine was adopted. The eyes were suddenly turned to a printed word, often the last word of a paragraph in a newspaper, and a stop-watch was started as the word was understood. The subject immediately tried to think of the opposite. The watch was stopped as soon as the auditory verbal image of the opposite was quite clear, or as soon as the search was abandoned as desperate. Immediately after stopping the watch and before looking at it, and before recording the opposite, the subject considered the following questions:

A. Did anything other than trying-to-think precede either the clear verbal image of the opposite or the abandonment of the task? If question A was answered "yes" then:

B. Did this something include a faint auditory image of the whole opposite word or phrase?

C. Did it include the first part of the opposite, occuring detached from the rest?

D. Did it include a feeling, not expressed in words, but meaning the same as "it is about to begin" or "something is arriving"?

E. Did it include a word or words proposed as opposite but rejected as unsuitable?

If the answer to A was "no" then the answer to B, C, D, and E were taken to be "no."

The results of 50 trials are given in Table 2. The least

TABLE 2

| Question | A | B | C | D | E |
|---|---|---|---|---|---|
| yes | 30 | 3 | 4 | 0 | 27 |
| doubtful | 2 | 0 | 1 | 1 | 0 |
| no | 18 | 47 | 45 | 49 | 23 |

time was 1 second, the greatest 28 seconds, the median 5 seconds. Visual images frequently occurred but were not systematically recorded. The image which was finally accepted as the right one,

[11] *Ibid.*, p. 182.

was occasionally heard earlier in some faint or abbreviated form (3 answers "yes" to B, and 4 to C) but usually it began almost as suddenly as a spoken word.

A description, at first sight appropriate, was that the imagery "had been rising continuously since the beginning of trying, and its sudden appearance was due to it crossing a threshold." To understand this more clearly a separate research was made on "Thresholds When Sensation is Regarded as Quantitative,"[12] and the conclusion was made that this threshold is essentially a beginning which goes differently in successive trials so that we like to represent its average progress by a "regression curve" passing at each instant through the mean intensity of imagery in many trials; that it would be impossible to prove that this regression curve took off from the time axis at an angle; and that it is quite as true, and mathematically much more convenient, to suppose that the curve rises without an angle as in Figure 1.

### 2*d*) *Thought, Imagery, and the Cut-off*

After drawing Figure 1, I became sceptical about the long flat top of the *m*-curve between 8.5 and 10.5 seconds.

*Experiment 1C.* This portion of the curve was re-examined by repeating Experiment 1A without distractors and trying to maintain the image of the opposite. Times were taken by a stop watch. It was found as the result of 15 trials that the auditory image of the opposite word came to an end in an ordinary conversation time, of the order of 1 second. If the word was still thought of, the auditory image often repeated itself many times without appreciable gaps; sometimes a vowel was long drawn out; sometimes relevant visual images alternated with the auditory images. This conglomerate is perhaps what some writers call "thought." If so, the thought is like jam containing in it certain recognizable berries which are like the images. And the cut-off shown at 10.6 seconds in Figure 1 is the cut-off of the thought, for individual images faded inevitably and could not be sustained by *v*.

### 2*e*) *The Technique of "Brief and Feeble" in Experiments on Forgetting*

By forgetting is here meant the unconscious process which begins with ceasing to think about anything, and ends with a difficulty

[12] *Brit. J. Psychol.*, 1928, Oct.

in recalling it. The experiments are for times usually less than one minute. Now Experiment 1 shows that trying-to-think, when it succeeds, has a persistent effect in shortening the delay at the repeated trial, so if we want to test the progress of forgetting we must:

(1) Make a suitably feeble effort to try to remember;

(2) Keep up this effort for only a brief time. Here 1.5 seconds has been allowed;

(3) Provide distraction during the period extending from ceasing to think up to the test;

(4) Take note of any uninvited remembrances which may occur, and regard such observations as suspect until the contrary is proved.

An established and elaborate technique, designed to eliminate from memory experiments the effects of various disturbances, is described by C. S. Myers.[13] But it relates chiefly to intervals of minutes, hours, or days, whereas the problem has been to adapt it to an interval of 10 seconds. Accordingly, various disturbances, such as fatigue, practice, special interests, and repressed complexes, have had regretfully to be left to be partially eliminated by mere averaging. From a comparison of the dots and circles in Figure 2, it looks as if the special precautions described in (1), (2), (3), (4), above, might be important; although there is an alternative ex-

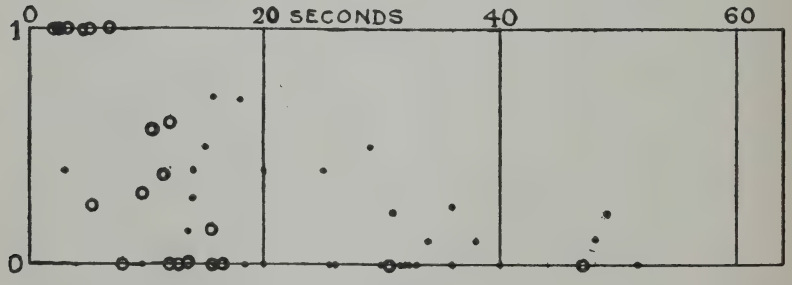


FIGURE 2
The time-scale is the same for Figures 2, 3, 4, and 5

planation (see 2*i*). Distraction has been recommended by G. M. Whipple[14] for a similar purpose.

[13]Myers, C. S. A text-book of experimental psychology. Cambridge: Cambridge Univ. Press; New York: Longmans, Green, 1911, 1925. See Vol. I, Chaps. 12 and 13.

[14]Whipple, G. M. Manual of mental and physical tests. Baltimore: Warwick & York, 1915, 1921. See Vol. II, p. 158.

### 2*f*) *Forgetting*

*Experiment 2.* An interesting book was read in the subject's ordinary way by visual perception, accompanied by auditory word images. The last line of the page, previously kept hidden, was noticed by reading it once only, but more slowly, and with close attention during 3 or 4 seconds. As the eye passed the midpoint of the last line a stop-watch was started. The reading was continued attentively down the next page until the end of the first paragraph, when the watch was stopped and the last line recalled as far as it came up quite easily within about 1½ seconds. Prolonged or intense effort to recall was purposely avoided. The diagram (Figure 2) shows the fraction of the words in the last line which were correctly remembered after various times of reading in 50 trials. A complication was that the words sometimes came up as auditory images before they were invited. Such observations are distinguished as dots from the circles which represent observations in which no uninvited images occurred; but the dot represents the voluntary remembrances subsequent to the uninvited.

Disregarding the dots, Figure 2 shows that all lines of words do not behave alike—yet alike in so far as the fractional number of words correctly remembered was unity up to 5 seconds, and zero beyond 16 seconds. The mean time of transition was about 9 seconds.

Of the intensity of the memory images, Figure 2 tells us nothing. By introspection it seemed at first that the words emphasized by silent, slow reading were either remembered in an ordinary conversational tone, or else not at all, never as a yell nor as a whisper. Repeated experiments, however, revealed images that developed rather more slowly than the rest. Once there was an unmistakable dimness, neither all nor nothing; this occurred at 10.4 seconds, about the time when Figure 2 shows that most synapses would be closing. Introspection thus gives curves like Figure 3.

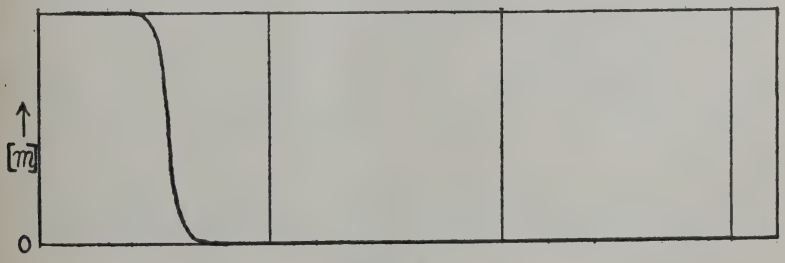


FIGURE 3

This 10-second period is possibly the same as the "specious present" of William James.

Whereas a single experiment of Type 1A gives the *whole* curve of Figure 1, a single experiment of Type 2 gives only two points on the curve of Figure 3, namely, the starting point at 0 seconds, and the variable finishing-point. The ordinate of this curve might be called "readiness for imagery," it being understood that readiness is measured by the intensity of imagery attained after not more than 1.5 seconds of feeble trying. A small square bracket as in [*m*] is used to denote readiness.

### 2*g*) *Fading and Recovery*

*Experiment 3.* This was like Experiment 2 except that the last line of the page was read 20 times. Usually the stop-watch was set going at the middle of the 20th reading. As a result of 49 trials it was found that the auditory imagery was usually most intense at the first reading. Understanding was at its maximum at the first to the fourth reading, according as the passage was obvious or subtle. Boredom was most intense at about the seventh reading; after that one became resigned, and imagery faded gradually. The fraction of the initial intensity to which it had faded at the 20th reading was estimated in 41 trials, as in Table 3.

TABLE 3

| Fraction of initial intensity | 0.1 | 0.2 | ¼ | 0.3 | ⅓ | 0.4 | 0.5 | 0.6 | 0.7 | 0.8 | 0.9 | 1.0 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Number of trials | 3 | 4 | 1 | 10 | 2 | 7 | 5 | 3 | 0 | 4 | 2 | 0 |

The median fraction is near 1/3 of initial intensity. After reading the last line for the 20th time, the subject continued immediately to read the next page in the ordinary way, but with close attention to insure distraction. Stopping the watch anywhere, the subject then tried briefly and feebly to recall the last line of the previous page. Figure 4 shows, for 39 trials, the fractional number of words remembered as a function of the time. The dots and circles have the same meaning as in Figure 2. The average time of forgetting is seen in Figure 4 to be about 30 seconds, as against 10 seconds in Figure 2.

The recovery from fading was studied in 14 of the aforesaid trials by estimating the intensity of the voluntarily recalled image as

FIGURE 4

a fraction of the maximum normal intensity. In this work a stop-watch was found to be too distracting, and lines of print were used as timekeepers. They were compared with the stop-watch by reading them again immediately afterwards. These 14 trials are each represented by one straight line in Figure 5. The numbers 0, 1, 2, 3, at the ends of these lines are the number of uninvited recurrences preceding the voluntary recall. All the words were remembered except in three of these 14 trials, and even in those more than half were remembered.

The estimates of auditory intensity were for the average of the words in the line of print from which individual syllables may vary.

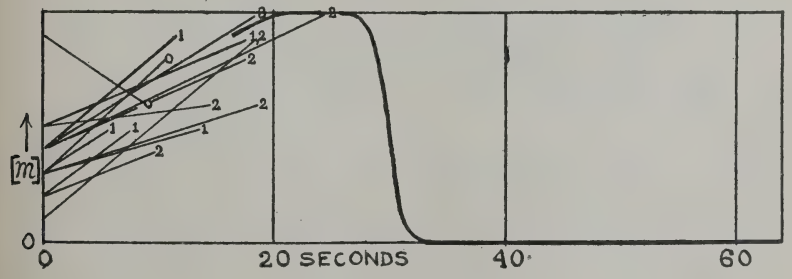


FIGURE 5

In Figure 5 we see two unconscious processes, first recovery from fading, then forgetting. Much use will be made of this diagram when formulae are to be fitted. But as several psychologists found it to be unconvincing, Experiment 3 has been repeated in 58 trials, spread over 4 months, since Figures 4 and 5 were drawn.

Twenty-two of these new trials were free from uninvited recurrences, and have been plotted in a diagram like Figure 4 (circles)

but not printed. The longest time at which all words were remembered was 26.6 seconds. The shortest time at which no words were remembered was 12.7 seconds. There were five of these trials, for which some but not all words were remembered, and the mean of their times was 23 seconds. This is an estimate of the "closing time." But it may be too small, as there were too few observations at longer times. The previous estimate from Figure 4 was 30 seconds.

For the purposes of Figure 5, on the contrary, we should accept only those trials in which all the words were correctly remembered after about 1.5 seconds of feeble trying. These 28 trials have been classified according to the number of uninvited recurrences in each of them, and are shown in Table 4. Each of these classes has been further subdivided into long times and short times, putting equal numbers of trials into the two compartments, and sharing equally an odd middle trial when it was present.

TABLE 4

| | | | | |
|---|---|---|---|---|
| Number of uninvited recurrences per trial | | 0 | 1 | 2, 3, or 5 |
| Number of trials | | 10 | 9 | 9 |
| Plotted on figure | | 5A | 5B | 5C |
| Mean intensity at 20th reading as fraction of maximum | | $0.3_0$ | $0.3_2$ | $0.4_2$ |
| Shorter times | Mean slope as fraction of maximum intensity per second | 0.052 | 0.027 | 0.027 |
| Longer times | Time of mean midpoint, seconds | 3.9 | 6.0 | 7.5 |
| | Mean slope as fraction of maximum intensity per second | 0.017 | 0.022 | 0.010 |
| | Time of mean midpoint, seconds | 8.7 | 10.4 | 14.5 |
| The numbers in the last 4 rows are plotted in Figure 5D as | | circles | dots | triangles |

Two alternative standards were employed as aids to estimating the intensity of the faded imagery. Thus in some trials, after reading the last line of the page 19 times, the previous line was read once and assumed to be an unfaded standard; then the last line was read for the 20th time during which the stop-watch was started. These observations are marked on Figures 5A, 5B, 5C, by single arrowheads. In other trials a less disturbing standard was arranged by sitting at such a distance from a gas fire or from a clock that its sound matched the intensity of unfaded imagery. These are marked by double arrowheads. In the future a telephone emitting a steady sound may be used, as in the research on "Loudness and Telephone Current."

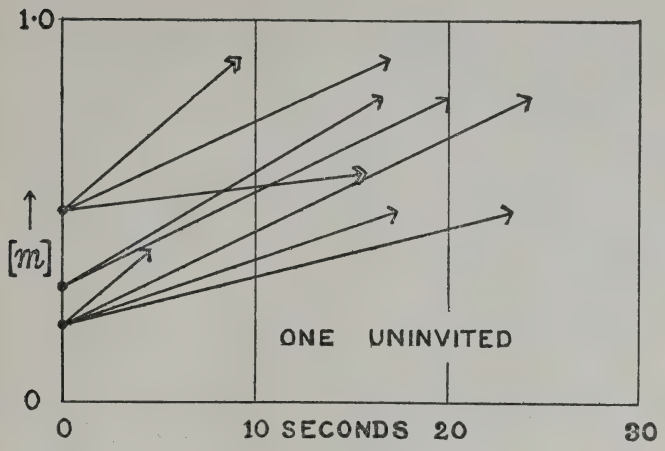


FIGURE 5A

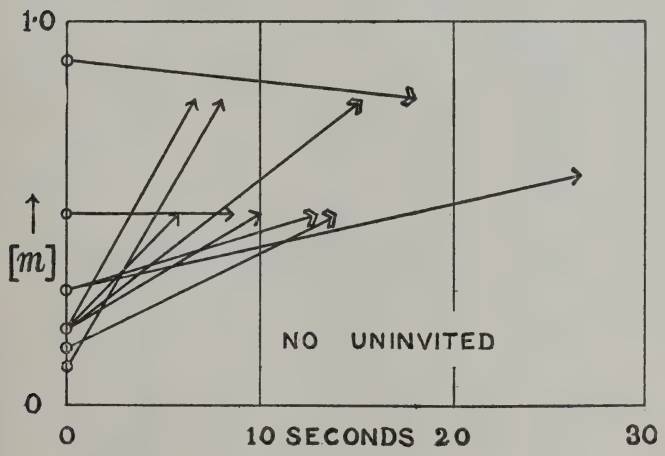


FIGURE 5B

For simplicity in Figures 5, 5A, 5B, 5C, the lines are drawn straight, but they probably ought to be curves convex upwards, for when in Figure 5D the mean slope is plotted against the time of the mean midpoint, we see that the mean slope decreases as time increases.

Do words or syllables recover completely from fading before

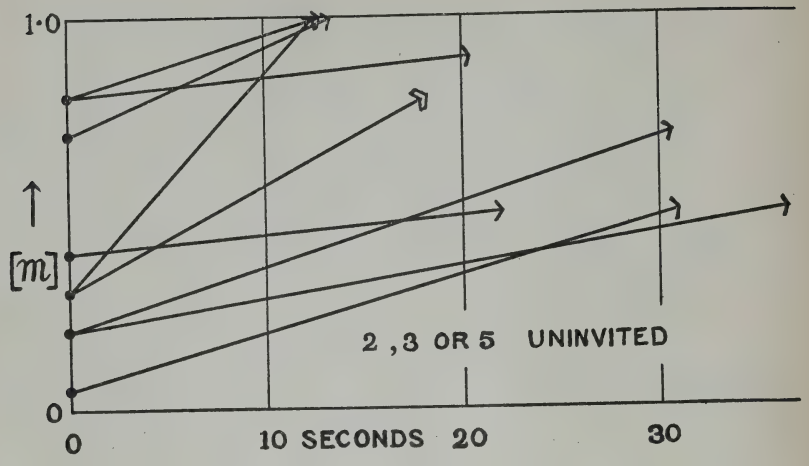


FIGURE 5C

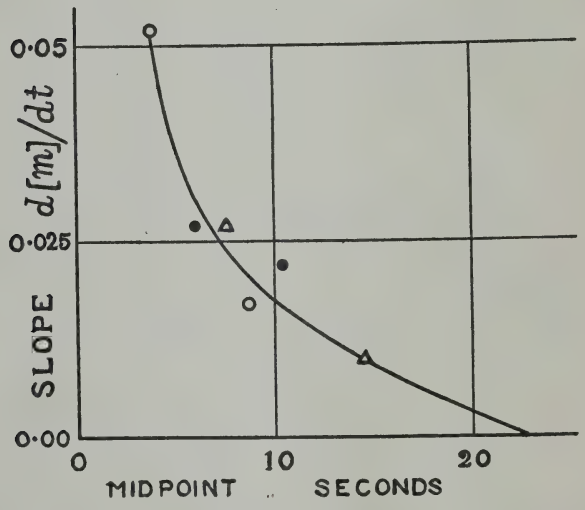


FIGURE 5D

they begin to be forgotten? In Figure 5C are three arrows which actually meet the top line, but in Figures 5A and 5B there are none. Some arrows are horizontal. Among these 58 new trials of Experiment 3 there are 15 in which some words of the line were forgotten

when the intensity of the remainder was estimated. These 15 trials have been divided into two groups according as the fractional number of words remembered was large or small. The group means are given in Table 5. From these facts it is evident that a whole line of words does not completely recover from fading before it

TABLE 5

| Number of trials | Fractional number of words remembered | Intensity of remainder as fraction of maximum intensity |
|---|---|---|
| 7½ | $0.7_8$ | $0.8_3$ |
| 7½ | $0.3_6$ | $0.7_7$ |

begins to be forgotten. As to individual words and syllables, the observations could be explained by incomplete recovery before forgetting. But an equally valid explanation would be that separate words all recover perfectly but that their closing times are scattered over 10 seconds, and each word is near to its maximum for a briefer time, say 1 second.

In Figure 5D a curve drawn through the circles, dots, and triangles, tends to cut the base line somewhere between 19 and 24 seconds. When it cuts, then $d\ [m]/dt = 0$, in which $[m]$ is the readiness for imagery. Then $[m]$ is stationary, actually a maximum. This time 19 to 24 seconds agrees roughly with the "closing time" of 23 to 30 seconds, estimated from the fractional number of words remembered.

### 2*h*) *Completer Fading*

*Experiment* 4. It was noticed in Experiment 3 that any slight pause in the 20 readings allowed the imagery to recover a little from fading. It seems probable that one word of the line was thus recovering while another word was being read. Therefore, to insure completer fading only one word should be repeated.

The word *jolly* was imagined auditorily 100 times in rapid succession, the eyes being closed. This experiment was far from jolly to perform.

### 2*i*) *Analysis of the Uninvited Remembrance*[15]

(1) Not every phrase tends thus to recur. Emphasis seems to be the cause.

[15]Similar processes have been observed in reflex arcs: Dreyer, N. B., & Sherrington, C. S. Brevity, frequency of rhythm, and amount of reflex nervous discharge as indicated by the reflex contraction. *Proc. Roy. Soc.* (*B*), 1917-19, **90**, 270.

TABLE 6

| Time from start (secs.) | Repetitions from start: Intentional | Unintentional | Intensity of auditory imagery | Word recalled |
|---|---|---|---|---|
| 0 | 1 | 0 | 1 (standard) | *jolly* |
| – | 20 | 0 | 0.5 | " |
| 110 | 100 | 0 | 0.02 | " |
| Now read a novel to distract | | | | |
| 720 | 100 | 13 | | " |
| 780 | 101 | ? | 0.05 | " |
| Distracting reading continued | | | | |
| 1920 | 102 | 21 | 0.6 | " |
| Distracting reading continued | | | | |
| 5100 | 103 | 38 | 0.6 | *often* then *jolly* |
| Meal, conversation | | | | |
| 7320 | 104 | 40 | | *often* then *hopeful* then after a few seconds *jolly* |
| Reading | | | | |
| 8580 | 105 | 40 | 1.0 | *jolly* after a few seconds of hard trying |

(2) *Experiment 5.* To study the time relations only the last word of a page was emphasized, and the time of each uninvited recurrence was recorded by electric key and chronograph. The book was *The Story of Prague* by Count Lützow (Dent,, 102). The last word was spoken aloud, the page was turned, and the reading continued.

TABLE 7

| Page | Word | Times in seconds from reading aloud to beginnings of successive occurrences | | | | | |
|---|---|---|---|---|---|---|---|
| 25 | does | 2.4 | 5.6 | 8.7 | 12.3 | 36.3 | |
| 26 | Prague | 3.3 | 8.1 | 12.5 | 16.0 | 21.2 | 29.7 |
| 30 | to | 2.8 | 6.6 | 9.7 | 16.3 | 26.5 | |
| | | (The word *took* occurred in the distractive reading and became entangled with the emphasized word *to*) | | | | | |
| 31 | the | 3.8 | 9.6 | 15.9 | | | |

The phenomenon differed from the familiar damped oscillation described by the linear equation

$\frac{d^2x}{dt^2}+2\epsilon\frac{dx}{dt}+\mu^2x=0$ (in which $\mu$ and $\epsilon$ are independent of $t$), because the pauses were longer than the disturbances, and because

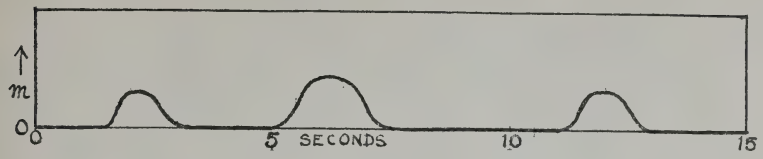


FIGURE 6

the second recurrence was often more intense than the first or third. Figure 6 is typical of the introspections.

(3) It may be suggested that the readiness for imagery oscillates. To find whether this was so, two experiments were made.

*Experiment 6A. Just after an uninvited image.* An uninvited auditory image occurred during distracting reading 2 seconds after the emphasis. As soon as the image had ceased I tried to recall it, and got it at once. This shows that the readiness for imagery had not decreased much. The experiment was repeated on 14 words, and the invited image, closely following the uninvited, was obtained 27 times, more times than the number of words because some words led to 3 recurrences. Once only was there confusion instead of the invited image.

*Experiment 6B. Just previous to an uninvited image.* This differed from 6A in that the observer tried to recall the word just before the second uninvited image was due. This experiment was much more tricky than 6A. It was tried 24 times with the results given in Table 8. Reviewing Experiments 6A and 6B, it is seen

TABLE 8

| | Number of trials |
|---|---|
| Story too interesting, experiment forgotten | 1 |
| No clear uninvited images, experiment impossible | 3 |
| Confusion came instead of invited image | 1 |
| Invited image obtained | |
| (*a*) just before uninvited | 9 |
| (*b*) but no uninvited followed | 4 |
| (*c*) coincident with uninvited | 3 |
| (*d*) mistimed giving sequence of Experiment 6A | 3 |
| Total | 24 |

that only twice out of 27 + 24 trials did the image fail to come when invited. This was during about 10 seconds after the emphasis. It is clear evidence that the readiness for imagery did not decrease much.

If there is a "refractory phase,"[16] it seems from these experiments to be shorter than half a second.

There was a remarkable difference in pleasantness between Experiments 6A and 6B. To have an invited image about 0.7 seconds *after* an uninvited image was as pleasant as repetition in music; but to have them in the reverse time sequence was often mildly perplexing, disconcerting, with a feeling of being bumped or knocked about inside the head.

(4) The disturbance is very conative, being, as well as the auditory image, a feeling like "it is important" or "soon I must," but vaguer and not expressed in words. In one experiment no image of the emphasized word appeared, but at 1.8, 6, 12, and 19 seconds there was a brief tension in the forehead and a desire to get on. The recurrences were more marked when reading an intellectual book *Research and the Land* by V. E. Wilkins than when reading *Labrador Days* by W. T. Grenfell or *The Constant Nymph* by Margaret Kennedy, which two are emotionally exciting. The latter were more effective distractors.

(5) The dots in Figures 2 and 4 lie to the right of the circles. Two alternative explanations (*a*) and (*b*) are possible:

(*a*) The closing-time varies greatly from causes that have nothing to do with the uninvited images, but delay in closing affords more chance for uninvited images to intervene.

(*b*) The conative urge which the subject tries unsuccessfully to prevent acts in the same way as intentional trying-to-think, in so far as both make memory persist.

Similarly, in recovery from fading, on comparing Figure 5A with Figure 5B and Figure 5C, it looks as if the uninvited recurrences delayed the recovery as in explanation (*b*). But when in Figure 5D we see the circles, dots, and triangles all near the same curve, it looks as if time were the only thing of consequence—as in explanation (*a*).

No way has been found to decide between the alternative explanations (*a*) and (*b*).

### 2*j*) *The Maximum Intensity of Imagery*

It has already been noted in Experiment 2 that words read silently in the ordinary way were usually remembered in an ordinary conversational tone, sometimes more faintly, but never louder.

[16]Lucas, K. Conduction of nervous impulse. New York: Longmans, Green, 1917. See chap. VI.

*Experiment 7.* This was like Experiment 2, except that, when taking note of the last line by eye, it was imagined as a yell. This was mainly auditory, but there were also muscular sensations or images near the eardrum. After distraction, the remembered image sometimes came up as a yell, sometimes in a conversational tone. Once the image was a yell without any words. This suggests that the image of a yell has a different mechanism from that of conversation. If so, we had better not mix them. The experiment was discontinued after 12 trials.

Experiment 8 was like Experiment 2 except that the last line was read aloud, and in 19 trials gave results much like Experiment 2, except that the duration of memory was slightly prolonged to about 15 seconds.

### 2*k*) *The Equilibrium or Threshold*[17] *Intensity of Trying-to-Think*

Is there a threshold such that if we try less intently imagery does not arise, however long we persist in trying; whereas if we try more intensely than this alleged threshold, then relevant imagery sooner or later occurs?

When thoroughly overworked, one may try for two hours and yet fail to solve a mathematical problem which comes out in five minutes after a holiday. In a dream one may even try persistently but fail to recall the name of a close acquaintance.

*Experiment 9. Threshold depends on difficulty.* On an afternoon when listless and drowsy, I found it impossible to persist in trying so feebly as not to succeed in recollecting the names of relations familiar since childhood. Yet a harder problem, the proof of the relation between latus rectum and the shortest radius vector of a parabola, which proof I had known for twenty-seven years, was sought after for several minutes without any apparent progress. On switching on two or three times the previous intensity of trying-to-think, the proof became obvious after a few seconds, and left me awake.

*Experiment 10. Estimates of intensity of trying.* It has been shown by Experiment 2 that readiness for imagery is near its maximum up to six seconds after the silent emphasis. Now stopping the distracting reading at the end of the first line of the new page, that is, after about three seconds, I planned to try so feebly as just

[17]The word threshold is unfortunately ambiguous. See: Richardson, L. F. Thresholds when sensation is regarded as quantitative. *Brit. J. Psychol.*, 1928, Oct.

not to recall the emphasized words in spite of persistent trying. It was difficult to be feeble enough. It could sometimes be managed by almost falling asleep while reading the distracting line. After 10 trials it was estimated that the threshold intensity of trying was 1/5 of the "feeble" trying of Experiment 2 and perhaps 1/15 of "examination pressure," which is about the same as the intense trying of the opposite test in Experiment 1. But this experiment, No. 10, is the most difficult and uncertain of Experiments 1 to 14.

*Definition.* Let the unit intensity of trying-to-think be defined to be the threshold value when readiness for imagery is at its maximum with respect to time in the course of recovery and forgetting. This unit is natural to the individual; but there is no obvious way of comparing the units of two individuals. When expressed in this unit let the intensity be noted by $V$.

The result of Experiment 9 is then that $V=5$ during Experiment 2, and $V=15$ in the early part of Experiment 1.

### 2*l*) *Rapid Alternations of Intensity of Attention*

*Experiment 11.* In contrast to the persistence for five or more seconds as in Experiment 1 and Experiment 10, I now tried to alternate from strong to feeble trying as rapidly as posible. It could be done in about two seconds. This fluctuating attention was applied to a memory which was at or near its maximum of readiness for imagery in the course of the curve of Figure 5. This was contrived by reading a book, noticing the last uncommon word of the page silently as in Experiment 2, turning to the next page, reading one line only, and then trying to remember; attention being in the sequence *strong-weak-strong* in some experiments, and *weak-strong-weak* in others. In 15 trials it was observed that the intensity of imagery usually went up and down with the intensity of trying. Simple proportionality of $m$ to $v$ seemed the best general description. But in a few trials $m$ was not variable or else less variable, as if $m \propto v^{1/2}$ certainly not to $v^2$. Also the changes in $m$ may have lagged a few tenths of a second after those of $v$.

### 2*m*) *The Behavior of Simple Vowels*

*Experiment 12. Speed of fading and recovery.* On trying intensely to experience the auditory image of *a* in *father*, the image faded almost to nothing in 20 seconds. This is faster than the fading of words. Recovery from fading was also faster, for it

occurred in five seconds during which *oh* was imagined as a distraction.

*Experiment 13. Persistent unfading images.* While in bed in a quiet room I imagined a vowel sound like *oo* in *good* very faintly, and kept it going for 200 seconds without any fading. In every in- or out-breathing the sound of the rush of air in the nostrils drowned the faint image, but it emerged again with its previous intensity.

Similarly, *a* as in *father* was kept going steadily for about 90 seconds at a feeble intensity. At greater intensities it faded. At less intensities it fell asleep, leaving the rest of me awake. There seemed to be only one intensity which could be kept steady for many seconds.

These persistent images may reasonably be supposed to occur when the subject adjusts his intensity of trying to the threshold value.

*Experiment 14.* But we have seen in Experiment 9 that for words the threshold increases with the difficulty of the thought. Accordingly, I tried to make the vowel image easy, by previous fading; thus: *a* as in *father*. Strong trying was maintained for 20 seconds during which the image faded to nothing. Immediately I dropped the intensity of trying to a sleepy attention. For two seconds there was no image, then it began rather suddenly and persisted for five seconds. This experiment on vowels seems at first sight to contradict Experiment 9 on more complicated relations. But they may perhaps be harmonized if we suppose that fading differs from forgetting by involving in the brain either a different chemical substance, or a different microscopic structure, or both.

### 2*n*) *A Connection with Physiology*

Since the present paper was first drafted, a work by Sybil Cooper and D. Denny Brown has been published.[18] Their Figure 9, showing the tension of a monkey's muscle as a function of time during and after the electrical stimulation of its cortex, has several resemblances to the course in time of the present introspections. Certain features of their observations and of mine are set out below in two columns so that corresponding features come in the same row.

[18]Cooper, S., & Brown, D. D. Responses to stimulation of the motor area of the cerebral cortex. *Proc. Roy. Soc.* (*B*), 1927, **102**, 222-236.

| S. Cooper and D. Denny Brown | Present Paper |
|---|---|
| (1) Steady series of break shocks applied to point of motor cortex. Rhythm 12.5 per second. | (1) Steady intensity of trying-to-think. Smooth not rhythmic. |
| (2) Tension of corresponding muscle. | (2) Intensity of relevant auditory imagery. |
| (3) Summation period 1.8 seconds in Figure 9. | (3) Period of trying before image comes. Shown as 7 seconds in Figure 1; but depends on the task. |
| (4) "If a stimulus be reapplied within 30 seconds after a previous stimulus, the summation period is markedly shortened . . . ." (p. 226) | (4) This shortening is shown on the right of Figure 1. It was conspicuous "especially if the interval between the two trials was not more than 15 seconds." |
| (5) Recruitment in Figure 9. Jerky with the rhythm of the stimulus. | (5) Gradual rise at about 7 seconds in Figure 1. Smooth just as $v$ is smooth. |
| (6) Suddenness of relaxation after stimulus stopped. Figure 9. | (6) Suddenness of cessation of imagery after trying-to-think stopped, shown at 10.6 seconds in Figure 1. |
| (7) Clonus. Figure 9. Period 0.5 seconds. | (7) Uninvited remembrance. Figure 6. Period 3 to 6 seconds. |

## 3. Mathematical Description

### 3a) *Leading Ideas*

It is not proposed here to deduce a theory from accepted principles. Instead, a set of equations will be stated as a pure assumption; their consequences will then be deduced and shown nearly to fit the experimental facts. The search for assumptions that have this property has been a long one. It would weary the reader were I to detail: what simplicities have been found inadequate; how theories that have perished have suggested experiments that survive; what hydraulic and electric models have been imagined and then eliminated; what elaborate calculations have been found to misfit the facts. But a few of the ideas, which suggested assumptions for trial, and have not appeared in Section 2 will now be mentioned.

(1) Formulae should not be complicated without good evidence.

(2) The introspected curves, Figures 1, 5, 6, are not reminiscent of the curves of that large part of physics which is described by differential equations that are linear.[19] Thus probably we must

[19]Riemann, G. F. B. (Ed. by H. Weber.) Die partiellen Differentialgleich ungen der mathematischen Physik. Braunschweig: Vieweg, 1910.

seek non-linear equations. In the related physiology, non-linearity is necessary, for Adrian[20] records that "The effects of two stimuli presented simultaneously or in rapid succession may be quite different from the sum of effects of each stimulus presented singly."

(3) We are conscious of having forgotten, but we are not conscious of the process of forgetting. Recovery from fading is likewise an unconscious process. So besides the introspectable variables $v$ and $m$ it is suitable to introduce non-introspectable variables.

(4) The shortest time with which we are concerned in introspection is, say, 0.05 seconds. The more rapid changes observed in experiments on nerves must somehow be smoothed out before they can enter a psychological description. Thus Adrian[21] gives reasons for believing that the number of impulses in a second in a sensory nerve is proportional to the intensity of sensation.

(5) McDougall,[22] whose *Physiological Psychology* was the original stimulus of the present research, has assembled many reasons for believing that important non-introspectable variables are the conductances of the synapses in the cerebrum. The present research began in 1918 by an attempt to translate into equations the properties of the synapse as listed on pp. 31-32 of that book.

Garnett[23] has made wide application of the synapse theory to education.

(6) Anyone who has switched on a Nernst lamp[24] and watched its dim persistent glow rising suddenly to splendor will probably admit that this process resembles the switching of attention on to a problem, the persistence in spite of little light, and at last a sudden appropriate image.

The Nernst Lamp behaves thus because its filament, like a spark gap, conducts more readily, the more rapidly energy is being dissipated in it. It would melt, if not stabilized by a series resistance. The dim foreglow is due to a separate heater.

("The Analogy between Mental Images and Sparks" is a title of a continuation of the present research.)

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[20]Adrian, E. D. The basis of sensation. London: Christophers; New York: Norton, 1928. See p. 117.

[21]*Ibid.*, p. 114.

[22]McDougall, W. Physiological psychology. London: Dent; New York: Macmillan, 1905. See also paper in *Brain*, 1901.

[23]Garnett, J. C. M. Education and world citizenship. Cambridge: Cambridge Univ. Press, 1921.

[24]Winkelmann, A. A. Handbuch der Physik. (3rd ed.) Leipzig: Barth. See Vol. IV, p. 674.

3*b*) *Formal Assumptions and Deductions*

The assumptions are equations (1) and (2)

$$M = V \frac{4S}{(S+1)^2} \tag{1}$$

$$k \frac{dS}{dt} = \frac{(SV^n - 1)}{(SaV^n + 1)} \frac{S}{(S+1)^2} \tag{2}$$

in which $t$ is time in seconds, $V$ is intensity of trying-to-think, $M$ is intensity of relevant imagery, both $V$ and $M$ being measured in units peculiar to the person, as will be seen; $k$, $n$, $a$ are "brain constants" independent of $V$, $M$, $S$, $t$. The non-introspectable variable is $S$, and it is very like the conductance of the synapse in McDougall's theory. But if we are not concerned with physiology then nothing need be assumed about $S$, beyond what is explicit in equations (1) and (2). We could eliminate $S$ by solving (1) in the form

$$S = \frac{2V}{M} - 1 \pm 2\sqrt{\frac{V^2}{M^2} - \frac{V}{M}} \tag{3}$$

and substituting in (2). But the result is cumbrous. So the presence of $S$ in the equations is not an extravagance, for it tends to economy of thought.

Let us now examine the consequences of these assumptions in a variety of special circumstances.

*Case 1.* $V = 0$, *that is to say, we are not attending at all to the words in question,* though we may be thinking about other things.

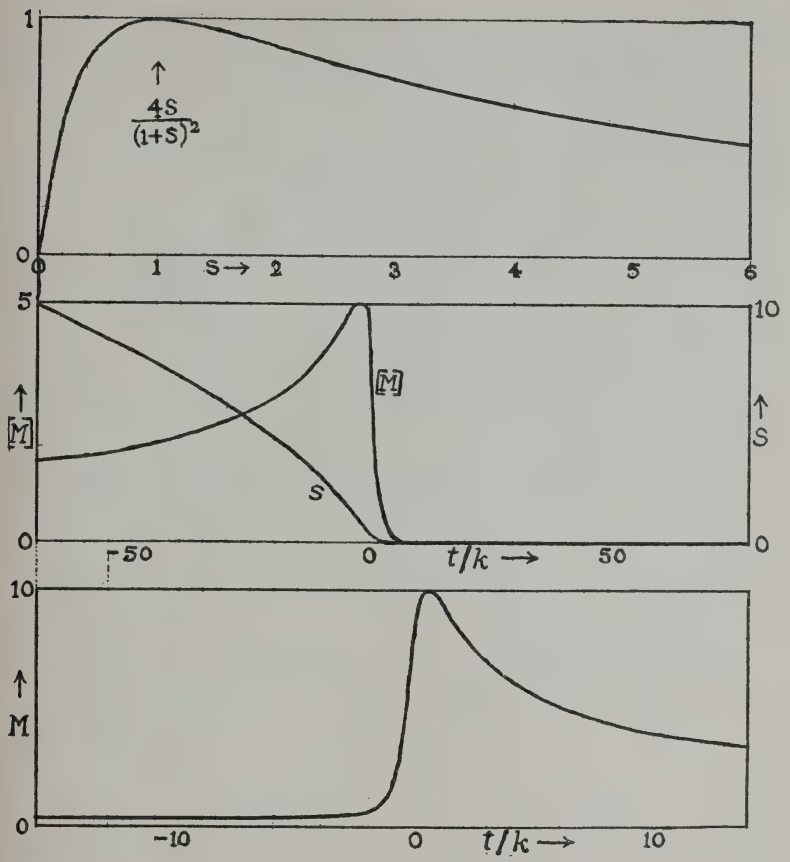
Equation (1) implies that $M$ vanishes when $V$ does. This is generally observed, but the uninvited remembrance of Section 2*i* is an exception; equation (1) does not describe it.

Again when $V = 0$ equation (2) becomes

$$k \frac{dS}{dt} = -\frac{S}{(S+1)^2} \tag{5}$$

The integral of (5) is

$$\log S + 2S + \tfrac{1}{2}S^2 = -\frac{t}{k} + \text{(arbitrary constant)} \tag{6}$$




FIGURES 7, 8 AND 9

The course of $S$ in time is shown in Figure 8. If it be supposed that at various stages of this course the observer tests progress by application of $V = 5$ for a time so brief (see the technique of 2*e*) as not seriously to introduce the terms in $V$ in equation (2) then the observed $M$ will be found by eliminating $S$ between (6) and (1). This readiness for imagery is thus found by substituting $V = 5$ in (1) and so is

$$[M] = 5\,\frac{4S}{(S+1)^2} \qquad (7)$$

The course of $[M]$ when $V = 0$ is also shown in Figure 8, which

should be compared with Figure 5. In both are seen the slow recovery from fading, the maximum, the sudden fall (forgetting), and the lingering faint remembrance represented by the asymptotic approach to zero.

Equation (6) shows that when $V$ is constantly zero and $t \to \infty$ then $S \to 0$. That is to say $S = 0$ corresponds to complete forgetting, small $S$ corresponds to moderate forgetting.

The early part of the $[M]$ curve in Figure 8 is concave where Figure 5D shows that it should be convex. In view of the uncertainty of the introspections, this defect is not disastrous. The time scale of Figure 5 agrees best on the whole with that of Figure 8, when $k$ is about 0.5.

The calculated maximum is perhaps no more peaky than the introspected would be if syllables were observed individually (see $2g$).

The completer fading of Experiment 4 is beyond the left edge of Figure 8. It was estimated in that experiment that potential intensity of imagery recovered from 0.02 times to 1.0 times its maximum in about 7000 seconds. The equations (1) and (5) would make the corresponding change occur $t/k = 20{,}000$. Hence $k = 7/20$ second.

*Case 2. $V$ is independent of time and not zero.*

The integral of (2) is then

$$t/k + \text{const.} = \tag{8}$$

$$S + = \tfrac{1}{2}aS^2 + \frac{(1+2aV^n+a)}{V^n}S +$$

$$(2+aV^n+V^n+\frac{1+2aV^n+a}{V^n})\frac{1}{V^n}\log_e\,(S-\frac{1}{V^n})$$

and in particular for $a = 0.2$, $n = 2$, const. $= 0$ together with intense trying represented by $V = 10$ (9)

we have

$$\frac{t}{k} = \frac{S^2}{10} + 0.412S + 1.22412\log_e\,(S-0.01) - \log_e S \tag{10}$$

and $M$ is found by eliminating $S$ between (10) and (1). This was done numerically. As a function of time $M$ is shown in Figure 9, which should be compared with Figure 1. In both we see the long, almost imageless suspense, the sudden rise of an image followed by slower fading. In Figure 9 the initial $M$ is not so small as it should be. To make the time scale of Figure 9 agree with that of Figure 1, it is necessary to make $k = 0.5$ or thereabouts. This is of the same order as $k = 0.5$ or $7/20$ deduced from the recovery from fading.

Comparing (10) with (6) it is seen that the time taken to revive an image under intense trying $V = 10$ is longer, according as the previous time, during which the image had not occurred because of inattention ($V = 0$), is longer. That agrees on the average with everyday experience, although there are many exceptions.

Again when $S$ is initially the same $S_0$, (6) and (1) show that the image may be developed either by intense effort in a short time, or by less effort spread over a longer time,[25] provided $S_0 V^n > 1$.

*Case 3.* $V = S^{-1/n}$ (11)

Then from (2) it is seen that $dS/dt = 0$ (12)

Hence by (11) and (1) it follows that $dM/dt = 0$ (13)

This corresponds to the threshold or equilibrium intensity of $V$ described in $2k$. According to (11) the threshold $V$ is larger as $S$ is smaller, that is to say, as the idea is more difficult. This agrees with the introspections on words, but not with Experiment 14 on simple vowels.

*Case 4. $S$ is in the neighborhood of unity.* (14)

A graph of $4S/(S+1)^2$ as a function of $S$ is Figure 7. It shows a maximum at $S = 1$, so that if $S$ lies anywhere between 0.6 and 1.6 we have approximately

$$M = V \tag{15}$$

And although by (2), $S$ will change, yet because of the maximum, (15) may remain nearly true for a short time. This is in rough agreement with the results of Experiment 11, except that it does not show a lag of a few tenths of a second.

*Case 5. The units of $V$ and $M$.*

Referring back to the definition of unit $V$ in Section $2k$ we see that to verify it from equations (1) and (2) we must first chose $S$ to be

[25] Cf. Spearman, C. E. The abilities of man. New York: Macmillan, 1927. See pp. 334-335.

such that readiness for imagery is at its maximum when $S$ varies. Hence from (1), $S = 1$; next we must insert this value of $S$ into the factor, the vanishing of which in (2) fixes the threshold, that is to say, into $SV^n - 1 = 0$. It follows that $V = 1$, as required.

This done, the equations themselves fix the unit of $M$. For when $V = 1$ and $S = 1$ it follows from (1) that $M = 1$. That is to say: The intensity of imagery is said to be unity when readiness for imagery is at its maximum in the course of fading, recovery, and forgetting, and when intensity of trying is adjusted to the very feeble threshold value. An image of unit intensity is accordingly quite faint.

## 4. Conclusion

Equations (1) and (2) may be said to score about 13 major agreements with introspection on words, as against 4 minor misfits. For simple vowels Experiments 12 and 13 could be described by the same equations with altered constants, but Experiment 14 misfits.

So far, equations (1) and (2) have been proved to describe only the auditory images of the author. But in view of the great range of facts coordinated by McDougall's synapse theory, with which these equations are allied, there is hope that with appropriate values of the brain constants $k$, $a$, $n$, the equations may extend to many sorts of images and of people. In particular, they could probably be used in describing sleep and hypnotism.

The experiments and equations are restricted to times less than $10^4$ seconds and indeed usually to less than 30 seconds, so that their relation to reminiscence and obliviscence over several days has not been studied.

The equations describe a mechanism which we are free to manage.

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## L'IMAGE, LA FACULTÉ DE VOLITION ET DE DÉSIR, ET LA CONDUCTANCE CÉRÉBRALE

(Résumé)

On montre que plusieurs des changements observés dans l'intensité de l'image auditive des mots sont décrits par les équations suivantes:

$$M = V \frac{4S}{(S+1)^2} \tag{1}$$

$$k \frac{ds}{dt} = \frac{(SV^n - 1)}{(aSV^n + 1)} \cdot \frac{S}{(S+1)^2} \tag{2}$$

où $t$ est le temps en secondes, $V$ est l'intensité de l'effort de penser, $M$ est l'intensité de l'image appropriée, et $k$, $n$, $a$, sont des "constants cérébraux" indépendants de $t$. Il n'est pas nécessaire de rien supposer sur $S$ que ce qui est explicite dans les équations, car $S$ est éliminé avant que les équations soient comparées à l'introspection. Mais $S$ ressemble beaucoup à la synapse de la théorie de McDougall.

Pour voir la signification des équations, considérons quelques exemples, donnant les phénomènes psychologiques entre crochets. Supposons que $V = 0$ (manque d'attention) pendant que $t$ s'accroît. Ainsi (2) montre que $S$ décroît toujours vers zéro (l'oubli); et (1) montre que $M = 0$ (l'oubli est un processus inconscient).

Quand $S \ll 1$ (presque oublié), si $V$ devient tout d'un coup égal à 10 et y reste (essai difficile persistant), les équations veulent dire que $M$ reste petit pendant quelque temps (incertitude sans image) et ensuite tout d'un coup s'accroît au maximum (problème résolu), et après, $M$ décroît toujours (devenant plus faible).

Les équations décrivent un méchanisme qu'on est libre de contrôler.

RICHARDSON

## EINBILDUNG, ANTRIEB, UND GEHIRMLEITUNGSKRAFT

(Referat)

Es wird gezeigt, dass viele der beobachteten Veränderungen in der Intensität der hörbaren Wortdarstellung in folgenden Gleichungen beschrieben werden:

$$M = V \frac{4S}{(S+1)^2} \tag{1}$$

$$k \frac{ds}{dt} = \frac{(SV^n - 1)}{(aSV^n + 1)} \cdot \frac{S}{(S+1)^2} \tag{2}$$

wobei $t$ die Zeit in Sekunden darstellt. $V$ ist die Intensität des Denkversuchs, $M$ ist die Intensität der bezüglichen Einbildung, $K$, $n$, $a$, "Gehirnkonstante" die unabhängig von $t$ sind. In betreff des $S$ braucht man nichts weiter anzunehmen als was in den Gleichungen erscheint, da $S$ ausgeschieden wird ehe die Gleichungen mit Introspektion verglichen werden. Doch $S$ ist der Leitungskraft der Synapse in McDougalls Theorie sehr ähnlich.

Um die Bedeutung der Gleichungen zu verstehen, betrachten wir einige Beispiele, mit Beziehung auf die psychologischen Erscheinungen in Klammern, Sagen wir $V = 0$ (Unaufmerksamkeit) während $t$ zunimmt. Dann zeigt (2), dass $S$ stetig bis zum Nullpunkt abnimmt (Vergessen); und (1) zeigt dass $M = 0$ (Vergessen ist ein unbewuster Vorgang).

Wenn nun, falls $S$ 1 (fast vergessen), $V$ plötzlich gleich wird und bleibt (anstrengendes dauerndes Versuchen), so deuten die Gleichungen an, dass $M$ längere Zeit ganz klein ist (Vorstellungslose Spannung) und dass $M$ dann plötzlich bis zum Maximum emporsteigt (gelöstes Problem), und dass darauf $M$ stetig abnimmt (verschwindet).

Die Gleichungen beschreiben einen Mechanismus den wir willkürlich beherrschen.

RICHARDSON

## ОБРАЗЫ, ПОБУЖДЕНИЯ и ЦЕРЕБРАЛЬНАЯ ПРОВОДИМОСТЬ.

(Реферат).

Многие из наблюдавшихся изменений в интенсивности слуховых словесных образов могут быть описаны следующими уравнениями:

$$M = V\frac{4S}{(S+1)^2} \cdot \ldots \ldots \ldots \ldots \ldots (1)$$

$$k\frac{dS}{dt} = \frac{(SV^n-1)}{(SV^n+1)}\cdot\frac{S}{(S+1)^2} \cdot \ldots \ldots \ldots (2)$$

в которых $t$ означает время в секундах, $V$—интенсивность интеллектуального усилия, $M$—интенсивность образа, а $k$, $n$—мозговые константы, независимые от $t$. О величине $S$ можно сказать лишь то, что выявлено в формуле, так как $S$ элиминируется до проверки уравнения самонаблюдением. Однако, $S$ очень близка к проводимости синапсов в теории Мак-Даугóлла.

Для того, чтобы разобрать формулу, мы приведем несколько примеров ее приложения, указывая в скобках соответствующие психологические явления. Если $V = 0$ (отсутствие внимания) при растущем $t$, то (2) указывает, что $S$ постепенно доходит до нуля (забывание), а (1) показывает, что $M = 0$ (забывание—бессознательный процесс).

Когда $S << 1$ (близко к забытому), $V$ внезапно делается равным 10 и остается таким (перманентные настойчивые пробы), уравнение указывает на незначительность $M$ для некоторого промежутка времени (состояние, лишенное образов) и что $M$ внезапно доходит до максимума (решение задачи) и уже потом уменьшается (угасание).

Уравнение описывает здесь некоторый произвольный механизм.

**Ричардсон (Richardson).**

# NOTES

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## TWO TESTS FOR HAND AND EYE COORDINATION

### A. DART-THROWING AT A MOVING TARGET

The target consists of a piece of cork carpet, marked with a "bull" of 1.5 cms. radius, surrounded by nine concentric rings, the radii increasing by 1.5 cms. The "bull" is picked out in white. The carpet is fastened eccentrically to a disc of three-ply wood, 30 cms. in diameter, and the whole is placed on the disc of a gramophone box. This can just be seen in Figure 1, below the lower edge of the disc.

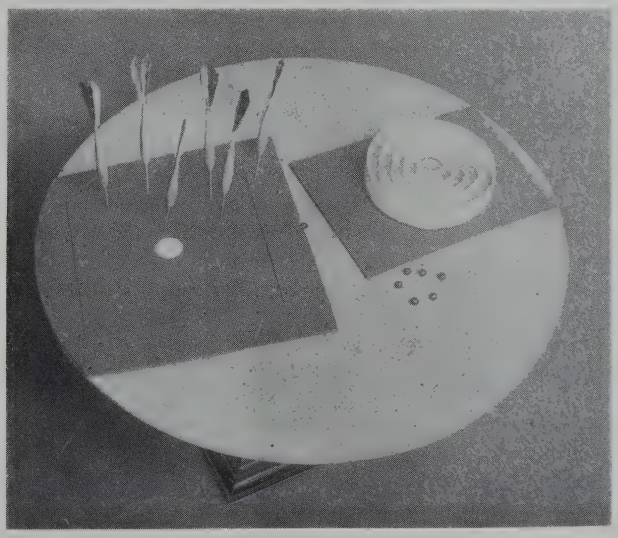

FIGURE 1
DART-THROWING AND BALL-DROPPING APPARATUS

The subject to be tested stands at a distance of 2.5 meters and throws darts at the target as it revolves.

The speed of revolution and the rate at which darts must be thrown can be varied as desired. In tests which are at present being applied, the speed is about 50 revolutions per minute, and six darts, held in the left hand, are thrown one at a time with the right hand, one at every revolution..

### B. Ball-Dropping onto A Moving Target

Large lead shot (8 mm. in diameter) are dropped, one at a time onto a target which consists of five concentric rings of cardboard; the radius of the "bull" is 1.5 cms., the radii of the other rings increase by 1.5 cms. The target is fixed on a circular disc of three-ply wood with the center of the "bull" at a distance of 15 cms. from the center. The disc is rotated by gramophone mechanism.

The subject, who stands naturally erect, with his toes at a distance of two feet from the center of rotation, holds six balls in his left hand and, with his right hand, drops one ball at every revolution onto the target.

The apparatus for these two tests was made for me by Mr. A. G. Sillitoe of this laboratory.

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## A "TIMING" TEST

This apparatus was designed to test that complex ability usually termed "timing." The ability consists, as a rule, in hitting a moving ball with an implement so that the ball is sent in some specified direction.

The apparatus is intended to reproduce the conditions in a novel way: a small metal ball is rolled down an inclined plane and across a horizontal sheet of plate glass; as it rolls across the glass, the ball is hit, in a direction at right angles to its direction of motion, with a striker, a thin cylindrical piece of wood held vertically in the hand.

The plate glass (*G*), 12″ x 24″, is supported at the two ends on pieces of wood which project beyond the sides of the glass and carry adjusting screws, one screw at each corner of the glass. The two nearest screws are marked (*A. S.*) in the figure; the far screws are hidden. At the far end of the glass are nine compartments (*C*), into the center one of which the subject is instructed to endeavor to hit the balls. The compartments are each 1″ wide. They are made of cardboard, with an inclined plane entrance which acts as a trap for the balls (Figure 2).

The compartments are protected from injury by a wood frame, 1″ wide, of three-ply wood (*W*), which carries thin steel wires exactly in front of the cardboard divisions of the compartments. In addition to the compartments, four partitions, $P_1$, $P_2$, $P_3$, $P_4$, are placed, two on either side of the glass. The balls are motor ball-bearings, ½″ and $\frac{9}{16}$″ in diameter. They

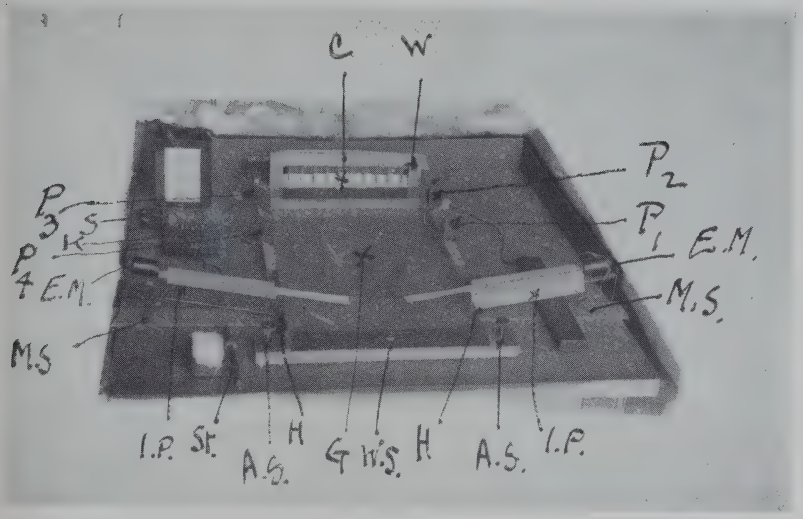


FIGURE 1
THE "TIMING" APPARATUS

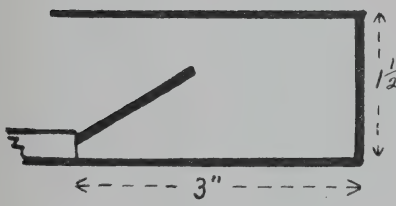


FIGURE 2
VERTICAL LONGITUDINAL SECTION OF A COMPARTMENT

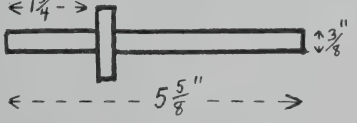


FIGURE 3
THE STRIKER

are allowed to roll down the inclined planes (*I. P.*) from the left and the right alternately. The planes, which are made of wood, are hinged at *H*, so that their inclinations can be varied. This is done by means of the movable supports (*M. S.*) The balls are held in position at the top of the inclined planes by electro magnets (*E. M.*) and fall when the circuit is broken by the switch (*S*). A commutator (*K*) in the circuit enables the direction of the current through the electromagnets to be changed, so preventing the permanent magnetization of their iron cores.

As the balls roll across the glass, they are hit by the subject, who is

seated at the near end of the table on which the apparatus is fixed, with the striker (*St*) which is held vertically over the white spot (*W. S.*) from the time the warning signal is given till the release of the ball. The striker, which is ⅜″ in diameter, is 5⅝″ long, and has a guard, 1¾″ from the bottom, which decides where it shall be held.

Marks are given according to the accuracy with which the ball is hit.

This apparatus was made for me by Mr. A. G. Sillitoe of this laboratory.

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## AN INDIRECT AIMING TEST

Many high-grade motor activities require that the hand, itself unseen, shall be put into a certain position which is dependent on the position in space of some other object which can be seen.

To test this ability the Indirect Aiming Apparatus was designed. It consists of a board (*C*), 38cm. x 14.5cm. x 1cm., which acts as a chin-

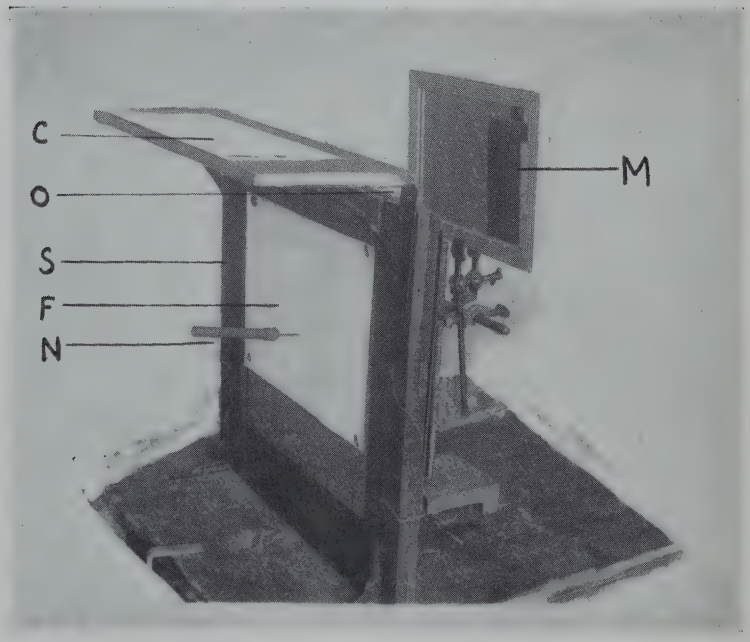


FIGURE 1
Indirect Aiming Apparatus

rest and which is supported by means of two iron brackets screwed onto a wooden screen (*S*), 38 cm x 38 cm. x 1 cm. The brackets are so fixed that the chin-rest (*C*) is raised 2.5 cm. above the top edge of the screen (*S*). There is, therefore, a small open space (*O*) 2.5 cm. wide, between *C*. and *S*. The screen is rigidly fixed by means of two brackets to a wooden base, which can be clamped to any table.

On the far side of the screen is a paper on which five targets, each 2

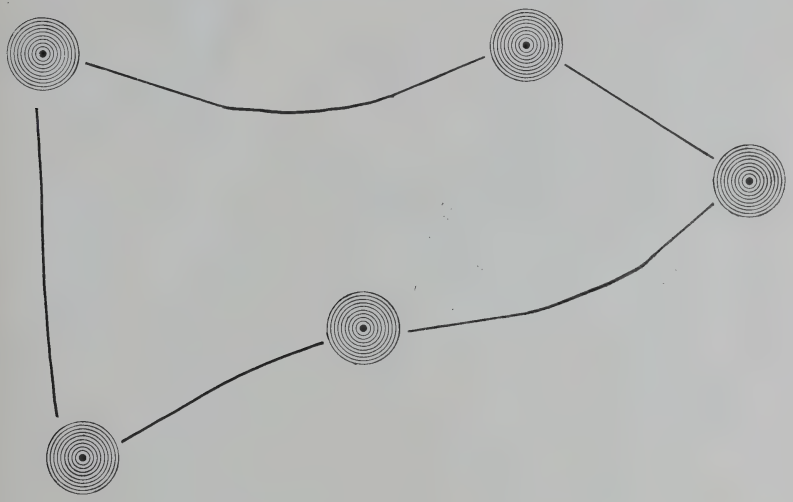

FIGURE 2
THE TARGETS

cm. in diameter, are printed. These can be seen by the subject when he places his chin on the chin-rest (*C*), in the mirror (*M*), which is at a distance of 6″ from the targets. On the near side of the screen, which is covered with cork carpet, a sheet of foolscap paper (*F*) is pinned.

The subject is required to prick the paper on his side of the screen, in the places which he judges are directly behind the targets, with the needle (*N*), which he holds by the wooden holder. He starts from a zero position, with the needle point in the center of the space (*O*), and returns to the zero position after every prick, going round the targets in a prescribed order. The speed is set by a metronome; that now being used is 138 per minute.

The tests are scored for consistency of aim and for absolute accuracy of direction of pointing.

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## A TEST FOR SHOOTING ABILITY

This apparatus is designed to test the tendency to cant the rifle when pressing the trigger.

It consists essentially of a pistol grip ($G$), and is so constructed that when the trigger ($T$) is "pressed" the upper marker ($M$), which moves in a vertical plane, records the movement on a smoked drum—not shown in the illustration (Figure 1)—while a simultaneous record of the "canting" is given by the marker ($M_2$) which indicates the amount the "gun"

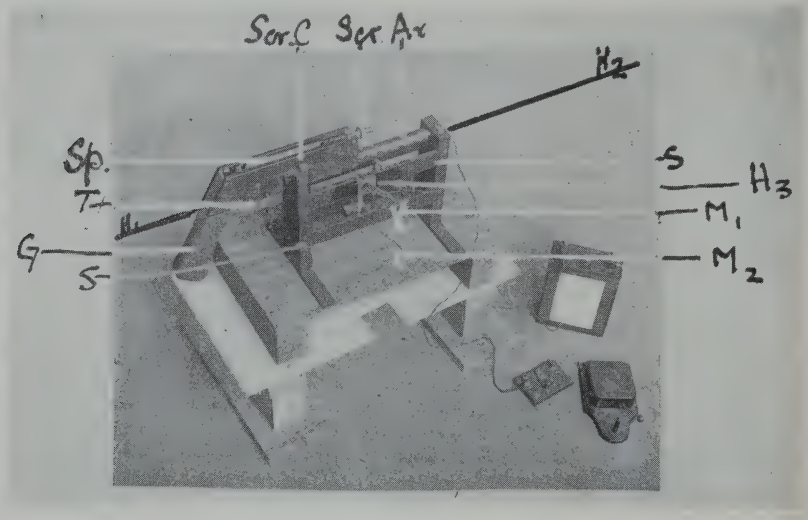


FIGURE 1
THE "GUN"

has been rotated to right or left round the axis $H_1H_2$.

The pressure required to "fire" is regulated by the Spring ($Sp$), the tension of which can be adjusted by means of the screw ($Scr$).

The "gun" is rigidly attached to the support ($S$), and has only one degree of freedom, viz., rotation round the horizontal axis $H_1H_2$.

The trigger works with an ordinary cam action (Figure 2), but the actual "feel" of the trigger is not smooth, as in a rifle. In this way the advantage the practiced rifle shot would have over the novice in the test is minimized.

The movement of the marker ($M_1$), which is pivoted at $H_3$ (Figure 1), is controlled by a cord which is attached to the shorter arm (Figures 1 and 2), passes through a hole in the axle ($Ax$), over a pulley ($P_1$) (Figure 2), under the adjusting screw ($Scr$), and to the pulley ($P_2$), to the rim of

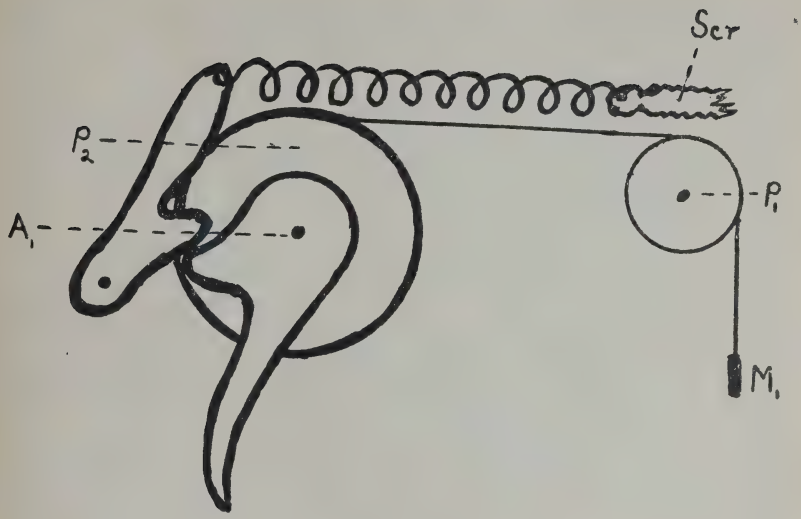


FIGURE 2
THE TRIGGER ACTION

which it is fastened. This pulley ($P_2$) is rigidly fixed to the trigger mechanism, and rotates with the trigger round the axis $A_1$.

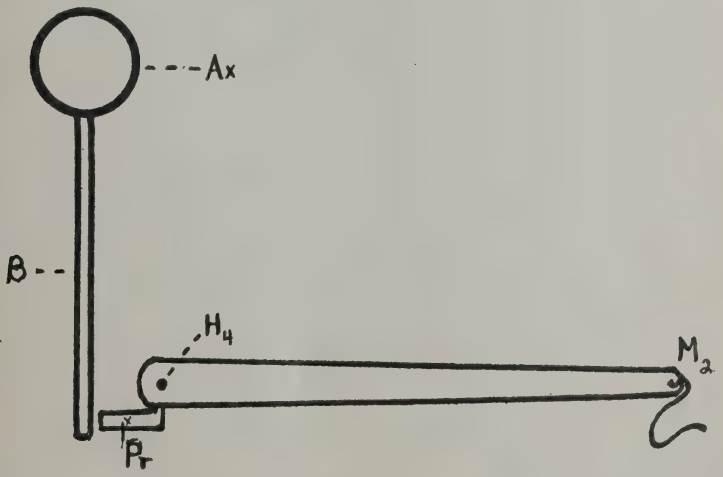


FIGURE 3
THE REGISTRATION OF THE "CANTING"

The movement of marker ($M_2$) is controlled by the steel bar ($B$) which is rigidly fixed at right angles to the axle ($Ax$) of the "gun," as shown diagramatically in Figure 3, an end-on vertical section.

The bar ($B$), rotating with the axle round the axis $H_1H_2$, gives a considerably enlarged movement to the projecting piece of metal ($Pr$), which is attached to the marker $M_2$ immediately below its axis of movement $H_4$. $M_2$ thus moves through a very considerable enlarged distance, about 8 cm., for a few degrees ($3\frac{1}{2}$) of rotation of the "gun."

The amount of "canting" which is possible is regulated by two screws, one on either side of the "gun." The one on the right side is marked ($Scr.C$) in Figure 1. That a subject may not unwittingly steady himself on these screws while pressing the trigger, a battery and electric bell are connected up so that the bell rings just before such a degree of canting occurs. This is obtained by attaching a wire, connected to one terminal of the battery, to the bar ($B$). As $B$ moves from one side to the other, the wire makes contact with the metal supports which are connected through the bell to the other terminal of the battery.

This apparatus was made for me by Dr. Schuster of the Industrial Fatigue Research Board.

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## A FORGOTTEN EXPERIMENT[1]

The phenomenon to which I wish to direct the reader's attention was first noticed by Purkinje. His data concerning it were corroborated later by Volkmann, and also by Fechner, who makes express mention of it in his work on binocular vision (1861).[2] Fechner remarks that, although personally he had not suceeded in seeing the phenomenon, it was observed by others during experiments performed on the "paradox" which bears his name.[3] The phenomenon was then, apparently, completely forgotten; at any rate no mention was made of it in treatises. In 1874, it was again noticed by Schön and Angelo Mosso.[4] These investigators had not known about it; because, as Schön points out in his controversy with Walb,[5] they had not found it mentioned either by Helmholtz or Hering. Oddly enough, the rediscovery of the phenomenon by Schön and Mosso was again forgotten; in none of the big treatises known to me do I find it referred to. True, the article by Schön and Mosso is registered in the list of works given at the end of the second edition of Helmholtz' *Physiological Optics,*

[1]Cf. *Arch. ital. psicol.*, 1920, **1**, 104.
[2]Fechner,, G. T. *Abh. d. math.-phys. Klasse d. K. S. Gesellsch. d. Wissensch.*, 1861, **5**, 427.
[3]*Op. cit.*, p. 416.
[4]Schön, W., & Mosso, A. *Arch. f. Ophth.*, 1874, **20**, 269.
[5]Schön, W. *Klin. Monatsbl. f. Augenhk.*, 1875, **13**, 356.

but no notice is taken of it in the body of the work. And yet the experiment is well worth remembering. It is, besides, so simple that without artificial means anyone may easily repeat it on himself at any moment. For this reason it lends itself admirably for demonstration in the lecture-room.

The object is to demonstrate the so-called rivalry between the visual fields of the two eyes. The experiment is as follows: One eye is closed and with the other the experimenter looks (without fixing any particular spot) at a surface of uniform color (a wall, the sky, a sheet of paper, etc.) and notices after a little time that there is a contest between the two fields of vision of which one is dark (closed eye) and the other light (open eye).

Concerning the details given by the various authors, I may point out that they are due to individual disposition, for which it is difficult to formulate a general rule. Purkinje, for instance, speaks of a swarm of white dots which he observed in looking at the sky, when the dark sensation of one eye invaded the light sensation of the other. This swarm of white dots in circular movement resolved itself finally into an uncertain, homogeneous, crepuscular light. Schön and Mosso, in their turn, mention having sometimes noticed during the darkening of the light field of the open eye a yellowish green or else a bluish tinge, at other times another undeterminable color. The fact that Fechner could not see the phenomenon at all must be attributed to some individual difference, as also that Purkinje succeeded only when he closed the right eye. Volkmann, further, could not see the swarm of white dots.[6] Personally, I see this swarm of dots but only when I look, as Purkinje did, at the sky, not when I look at a wall or a sheet of paper. Colors, however, I do not see. It is a matter of indifference to me which eye I close, and several persons on whom I experimented were like me in this respect.

According to Schön and Mosso, the oscillations occur at regular intervals, but the term must not be taken too literally. What is regular, according to my observations, is the contest from a general point of view; the surface on which the eye rests is seen alternately darkening and clearing, but the intervals themselves between the changes are not regular, that is, they are not of a periodic nature. During experiments which I performed on myself to this end, in which I rested the right eye on a wall of a light yellow color at a distance of four meters, I got intervals varying from 10 to over 40 seconds. These determinations are interfered with, in my case, by the difficulty I find, given the lack of a fixation point, in suppressing, during the darkening of the light visual field, an involuntary ocular movement which affects the sensation and has undoubtedly an influence on its duration. For this reason, I gave up the attempt to

[6]Fechner, *op. cit.*, p 427.

determine accurately the arithmetical averages of the durations of these intervals.

The single values, especially at starting, are different when the open eye is exposed to the light of the sky. These initial values are also not the same for both my eyes. Whereas I observe the first change when looking, for instance, at a wall after about 20 seconds (left eye) or about 40 seconds (right eye), the initial values when I look at the sky are from about one ot two minutes. This agrees in the main with Purkinje's results. Purkinje, looking at the sky with his left eye, noticed the darkening of the light field after "a few minutes."[7] During a few sittings devoted to establishing these initial values (that is, the duration of the first light phase) I noticed their tendency to diminish gradually. Thus, at one sitting, looking at the yellow wall above mentioned, other conditions being the same, I got the following initial values for the right eye: 42, 29, 37, 27, 19, 19, 35, 24, 24, 17 (seconds), and for the left: 22, 21, 13, 20, 20, 14, 9, 10, 10, 12 (seconds). From these results it is evident that both the peripheral organ and the whole psychic disposition adapt themselves gradually to these observations. The fact that in my own case the values for the right eye are in general longer than those for the left may be due to some individual characteristic.

The experiment may be further modified in this way: Both eyes, normally closed, are turned towards a clear daylight sky. A little pad or black handkerchief is then placed over one of them. Two visual fields appear, one dark, the other reddish gray. The effect under these conditions is the reverse of what obtains in the experiments of Purkinje, Schön, and Mosso. Whereas in their experiment, the light sensation first holds the field; in the present case, this is first held by the dark sensation. It is the dark field which is invaded by the lighter field, whereas in the older experiment the lighter field undergoes invasion by the dark one. Otherwise, the contest between the two fields takes place in much the same way.

As for the general results, they fully bear out what I have said in other articles; namely, that the contest in the whole series of achromatic sensations is of a slow nature.

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(Translated by E. L. Kiesow)

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## SUGGESTION IN PICTURES

Several factors may influence the preference rating of a picture. In an artistic judgment the picture itself, its form, colors, composition, etc., will undoubtedly form the most important item, but of significance may

[7]Fechner, *op. cit.*, p. 427.

be other factors: the presence or absence of a well-known painter's name, the type of frame, the proximity to other pictures and methods of display, the sale cost (previous and prospective), possible attached paragraphs which praise or defame the picture, etc. While each of these items might in turn be put to a scientific test, this paper presents an attempt to experiment solely with the effect of the descriptive paragraph.

Ten pictures were obtained. Care was taken to employ only pictures by unknown painters and to insure a considerable range of preference. A suitable title for the picture and a name for the artist were sought. Two paragraphs were devised for each picture and were attached to the mats. One set of paragraphs (designated as +) attempted to augment the ratings, the other (indicated with—) to facilitate a lower rating.

The subjects were not forced to read the paragraphs. Verbal reports indicated that certain subjects had read all the paragraphs, others had carefully avoided them, while still others had read or skimmed varying numbers. The + and—paragraphs were chosen in a random order to make up two sets, each containing five +'s and five —'s. While each rater viewed all ten pictures, he could read only one set of paragraphs.

The directions were as follows: "You will see a number of photographic reproductions on this table. Each one bears a number. You are to give your judgment in the following manner: on a sheet of paper put down the numerals 1-10, indicating the reproductions of corresponding number; then use the following symbols for your judgment:

1—very beautiful
2—pleasing
3—indifferent
4—displeasing
5—very unattractive

You are to judge the painting, not whether the reproduction is good or not."

Table 1 gives the ten titles and artists (?) with the corresponding two sets of paragraphs:

TABLE 1

| Picture | +Paragraph | —Paragraph |
|---|---|---|
| 1. "Russian Girl" by Vladimir Dolorski | "This portrait of a sunny girl of the Russian country is a magnificent example of the art of Dolorski, now the most prominent of all Russian painters." (from P. Michael Stradinski, *The New Russia,* p. 146) | This artist is a pupil of the greatest of all Russian painters, Vogul Sarofka; and though lacking the brilliancy of his famous master, his work is fairly well received by the critics. |

| | | |
|---|---|---|
| 2. "A Quiet Street" by Adriaen Vermeer | "Undoubtedly the master's most important work, it distinguishes itself by great delicacy of coloring and beauty of composition." (from Alfred Wisman, *The Seventeenth Century,* p. 87) Sold by Gloucester Co., New York to Henry Vanderloon for $120,000. | An interesting painting by a student of the Royal Art School of London. It was awarded third prize in a recent contest at the British Museum. |
| 3. "Self Portrait" by Van Gogh | Considered as one of the great master's best paintings, this remarkable piece was sold in 1925 by Paul Desjardins, Paris, to the Imperial Museum, Berlin for 210,000 francs. | This painting is looked upon as a poor sample of the master's talent, and at a recent sale in New York did not bring more than $350. |
| 4. "Fruit" by Emilo Carlo Casano | "The rich colors of the tempting fruit, tastefully arranged on the table, accompanied by a typical Italian wine-jug, have been perfectly rendered by this young artist, now so in vogue in Europe." (from D. Molino, *Italia,* p. 214) | One of the paintings that was presented in partial fulfilment of the entrance examination of the Acadèmia Royale de l'Arte di Roma in 1924. |
| 5. "Summer" by Karel Van Doren | The masterpiece of this distinguished artist. It hangs in the Musée de Luxembourg, Paris and is one of the most treasured possessions of this wonderful collection. | An interesting little sketch, evidently made in a spare half hour, which, though lacking the brilliancy of most of his work, pleases the eye. |
| 6. "Breath of Spring" by Desidere Lafayette | A brilliant specimen of the work of the youngest member of the Académie, who was elected to this distinction on the strength of his romantic *artistry.* | A clever sketch from one of the students of the Salon de Paris. It was awarded third prize at the annual exposition in 1926. |

| | | |
|---|---|---|
| 7. "Sundown" by Larinski | "A glorious expression of the spirit of the Russian country, with its golden sunset and its vast plains." (from S. Lazarus, *Continental Painters,* vol. II, p. 17). Dreyfus & Co. in Chicago sold it in 1926 to Mrs. Z. Cohen, New York, for $75,000. | Representative of the Russian school of the beginning of this century, this painting, once a great favorite with art critics, has fallen into oblivion with its creator—to make place for "the new art." |
| 8. "Music" by Hermann Russover | "In intimacy of conception, in ingenuity of composition, and in delicacy of treatment, this painting would be worthy of the brush of our greatest masters." (from Bernard Grunder's sales-catalogue, 1926; it was sold for $45,000 to Count Von Liederlich-Affenau). | As an imitation of the old masters the artist has created a painting with lighting effects à la Rembrandt, which, because not combined with the latter's artistic genius, fails to impress us as a truly great work of art. |
| 9. "Chinese Girl" by Esther Hunt | "The painting that made Miss Hunt rise from obscurity to *Who's Who,* a work of a beautiful design and a brilliant richness of color." (from the sales-catalogue of Messrs. Bryan, Bryan, & Bryan, London; they sold it to Mrs. Paul Dermoth, New York for $80,000). | An interesting painting by a little American high-school girl in Shanghai. It was awarded second prize at the annual exhibition of the Kai-Hung-Pau High School. |
| 10. "Romanesque" by Paul Rubens | "A painting of exquisite coloring and great beauty of composition. Particular attention is called to the richness of the fabrics and the delicacy of treatment of the subject." (from P. Jerome, *Masterpieces,* 1926, p. 304). The picture was last sold by Christie, London, for $240,000 to Mr. L. P. Van Doren, New York. | A first attempt of Mr. Reubens to get away from the influence of the French school. It was refused by the Salon last year, and as yet the artist has not received much recognition. Yet, there are many excellent points in his work. |

The results of the experiment, based on the performance of 64 university students from classes in experimental psychology, are given in Table 2.

TABLE 2

| Picture | Mean+ | Mean— | M.V.+ | M.V.— | Chances in 100 |
|---|---|---|---|---|---|
| 1 | 2.41 | 2.87 | .81 | .63 | 98 |
| 2 | 2.06 | 2.19 | .53 | .59 | 76 |
| 3 | 3.15 | 3.62 | .87 | .97 | 95 |
| 4 | 3.44 | 3.81 | 1.24 | .74 | 88 |
| 5 | 3.31 | 3.35 | .82 | .86 | 55 |
| 6 | 2.12 | 2.78 | .63 | .80 | 100 |
| 7 | 1.78 | 1.84 | .68 | .92 | 60 |
| 8 | 2.28 | 2.59 | .66 | .77 | 91 |
| 9 | 1.50 | 2.00 | .60 | .47 | 100 |
| 10 | 3.22 | 3.84 | 1.04 | .77 | 99 |

From this table it may be seen that in all cases the + series obtained a lower score (were ranked higher) than the —. It seems reasonable to assume that if all subjects had read the entire ten paragraphs the effects of the suggestion would have been even more striking. The rank correlation (rho) between the + and — means is .92, which indicates that in practically all cases the picture which was liked best in the + series was also preferred in the — group. The "Chances in 100" indicate the reliability of the mean differences (50 chances in 100 meaning that the found difference in means is not significant). Thus the differences in the means are quite significant in all but pictures 5 and 7. It is interesting to speculate on the failure of these two.

In summarizing, one might state that it has proved possible to influence the preference ratings of pictures to a considerable extent by means of attached and suitably composed descriptive paragraphs.

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## A NOTE ON MAZE LEARNING WITH THE TIME FACTOR CONSTANT

When comparing experimental results on two or more groups, we prefer if possible to express our conclusions in terms of a single variable. However, maze results are given in three variables: trials to learn, errors, and time. Usually there is general agreement among these, but, when the proportions are different or when there is disagreement, either the results are indecisively interpreted or one criterion is arbitrarily assumed to be the most valid index of learning ability. Trials and errors are largely interdependent, so the divergent element is usually but not always time.

Further, there is usually a large variation within groups which makes comparisons uncertain, particularly if the groups are fairly near alike in

ability. This is largely due to the varied methods employed in learning. A subject using a motor method will require many trials, but each will be of short duration, while one who learns by a verbal or counting method learns in fewer trials, each comparatively long.

Therefore, it was hoped that by making all subjects learn the maze following the beat of a metropome a twofold result would be accomplished: (1) The time variable would be eliminated; (2) Groups would become more homogeneous through being forced into more nearly the same general methods of learning, since each person does each trial in the same amount of time.

## Technique

Twenty subjects, university students, learned to the beat of a metronome a Warden maze (3), the pattern being a duplicate of his, with the important modification that it was a high relief pathway, devised by Miles (2). The results are compared with those of a control group who learned in the usual manner. More extensive results for this group are given in a previous article by the writer (1). The metronome sounded 100 beats per minute, a rate which was found to be optimal by experimenting on a few practice subjects who did not participate in the later experimentation. The subjects were required to traverse one unit of pathway to each tick, and in a correction to cover only one-half the length of the horizontal section, which is the length of two units, in one beat. Thus under all circumstances the same distance was traversed in one beat, in order not to break up the rhythm. A perfect trial would be done in 16 3/5 seconds, errors, of course, adding somewhat to the time required, since more distance would be covered. Brief directions explaining the general nature of the maze were given before starting. These were identical for both groups, with the necessary addition for the metronome group of explanation of how to follow the beat. The Miles (2) standard blindfold was used. The criterion of learning was three successive errorless trials, or three correct out of four.

## Results

1) *Trials to Learn.* In respect to trials to learn it may be seen from Table 1 that learning with the beat of a metronome is uneconomical. However, the additional items to be kept in mind and the necessity of

TABLE 1
Trials to Learn

| | Trials | $\sigma$ | Range | $\sigma$/m |
|---|---|---|---|---|
| Control | 16.7 | 7.5 | 5 – 31 | .45 |
| Metronome | 26.8 | 12.5 | 10 – 62 | .47 |

hurrying in the first trials, before adaptation to the task is perfect, can be assigned as reasons for this. The fact that the variability coefficient

is the same for both groups shows that by this measure at least the attempt to reduce the large variation was not successful.

2) *Learning Curves* for the two groups are very typical and similar to each other, that for the control group always being lower. The difference is more pronounced late in the learning, possibly indicating that the beat hastened choices when learning was nearly complete, yet still uncertain.

3) *Individual Errors.* The totals for the two groups, with the direction of correct choice, is given in Table 2. The correlation is $\pm.91\pm.06$, which

TABLE 2
TOTAL ERRORS ON EACH BLIND

| | 1 R | 3 L | 5 L | 7 L | 9 R | 11 R | 13 L | 15 R | 17 R | 19 L |
|---|---|---|---|---|---|---|---|---|---|---|
| Control | 53 | 25 | 105 | 165 | 27 | 83 | 85 | 46 | 148 | 38 |
| Metronome | 49 | 23 | 120 | 259 | 25 | 188 | 162 | 53 | 223 | 59 |

shows very close agreement. The piling-up of errors in a sequence of like choices, previously reported in detail (1), is seen also in the metronome group, in fact in accentuated form; that is, the ratios increased with much greater rapidity. This accentuation was found to be a characteristic of those subjects in the control group who reported having used a motor method of learning. A much greater tendency toward persistent errors was seen in the metronome group than in the controls. Choice was hurried, and habit, probably determined through recency and frequency, determined the direction. The necessity of following the beat resulted in very few corrections being made before coming to the end of the blind.

4) *The Reliabilities* of the two techniques are given in Table 3. All figures are corrected for halving the data. In this respect, then, the

TABLE 3
RELIABILITIES

| | Odd vs. even trials (6th through 15th) | First vs. second half of learning |
|---|---|---|
| Metronome | +.91 | +.91 |
| Control | +.95 | +.95 |

metronome technique is slightly inferior, although the reliabilities in both cases are very high as compared with many other maze results.

## DISCUSSION

Several points for and against this technique should be touched on. (1) In addition to the proportions of errors showing a tendency toward the motor type, a greater number of subjects reported themselves as having used it as a means of learning, 9 as against 6 in the control group. Two reported spontaneously that the metronome so hurried them that the verbal

or counting method was impossible, so they were forced to learn by motor means. (2) The tending of the errors into the motor type shows that the purpose of this experiment was partially fulfilled. Failure to reduce the variation might be accounted for by the added distraction of the metronome, which set a harder task for the subjects, thus bringing out individual differences to a greater extent than in the control group. (3) Not all subjects followed the beat perfectly, but there was no objective check to determine the degree of inaccuracy. However, subjects were very co-operative and after a few trials followed very well. (4) The fact that the average number of trials to learn was increased should not be regarded as evidence of a poor technique—in this case a more difficult task was set the subject, and thus learning was spread out over a somewhat longer period.

## Conclusions

In an effort to eliminate the time variable and to reduce the large variations within the group, 20 subjects learned a maze with the pace set by the beat of a metronome. The results are compared with those of a control group, which did the same task without the metronome. The variability was not reduced, but a more uniform method of learning was brought about. The characteristics of learning were not altered. It is hoped that this technique contains some suggestions which may be followed up more completely and successfully later.

## References

1. Husband, R. W. Human learning on a four-section, elevated finger maze. *J. Gen. Psychol.,* 1928, **1**, 15-28.
2. Miles, W. The high relief finger maze for human learning. *J. Gen. Psychol.,* 1928, **1**, 3-14.
3. Warden, C. J. Primacy and recency as factors in cul-de-sac elimination in a stylus maze. *J. Exper. Psychol.,* 1924, **7**, 98-125.

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## AN IMPROVED PHOTOMETER FOR MEASURING BRIGHTNESS THRESHOLDS IN HUNDREDTHS OF A METER CANDLE

The apparatus described in this paper was developed for the purpose of obtaining comparative data on differential thresholds for light intensity by means of the Equality of Brightness Method. During the course of its building a number of out-of-the-ordinary refinements were devised which resulted in the production of an instrument of unusual accuracy.[1] (1)

[1]Dr. Morris F. Weinrich, of the Physics Department of Columbia University, not only made the photometric measurements of the light bulb used but gave so freely of his time and suggestions in connection with general construction and wiring that the instrument could not well have been perfected without his help. The writer feels deeply indebted for this kindness.

Differences in color between the standard and comparison stimuli were effectively eliminated without the necessity of introducing flicker.[2] (2) A dark room was not required in which to operate the photometer but reliable readings were obtainable in fully illuminated surroundings. (3) Careful calibration permitted the immediate conversion of threshold values into meter-candle units. (4) Readings could be obtained either graphically or from a linear scale. (5) The photometer could be used as desired with the Method of Average Error, the Method of Minimal Change, or the Method of Constant Stimuli. In view of the possible value to other investigators of these and other less outstanding features it seemed advisable to publish a brief report of the instrument.

### A. Mechanical Construction

The photometer was built in two stories or levels contained in a light-proof wooden box, 26 inches inside length by 10 1/2 inches inside height by 10 1/2 inches inside width (Figures 1 and 2).

1) *Upper Level.* The Woodworth plan of using one source of illumination (Figure 1) and reflecting this by means of mirrors (*M, M', M'', M'''*) to two white comparison surfaces was adopted instead of employing two separate light sources.[3] The lamp selected was an ordinary 21 candle-power automobile headlight bulb taking 2.5 amperes at 6.5 volts. This type of lamp was chosen because its unusually short filament (about 2mm.) was not likely to sag or change length, and because its sturdy construction reduced the possibility of its being damaged by jarring.

The bulb was completely enclosed in a circular screen (*C,* Figure 1)

[2]The error resulting from uncontrollable color differences has been the source of by far the greatest criticism of the Equality of Brightness Method as opposed to the Method of Flicker. See: Ferree, C. E., & Rand, G. A preliminary study of the deficiencies of the Method of Flicker for the photometry of lights of different color. *Psychol. Rev.,* 1915, **22**, 114.

[3]The principal advantages of this design were threefold. (1) The elimination of possible color differences in the two reflecting screens has already been mentioned. (2) Slight fluctuations in voltage, which in the case of two bulbs might affect the filaments of each lamp differently, were eliminated when one source was employed. (3) If the filament of the single lamp changed by a small amount during the course of the experiment, this change was less likely to act differentially on the two illuminated surfaces than would be the case were two separate bulbs used.

Against these advantages rested the disadvantages of using four mirrors, whose reflection factors were probably not alike. The reflection factor remains virtually the same, however, for any given mirror, that is, it is constant (within a reasonable length of time). Consequently, once the center position of the light for our instrument had been empirically determined, differences in the reflection of the mirrors no longer needed to be taken into consideration. The Woodworth plan also eliminated the possibility of using a fixed standard light—the standard varying concomitantly and inversely with the comparison. No difficulty, however, was experienced on this score.

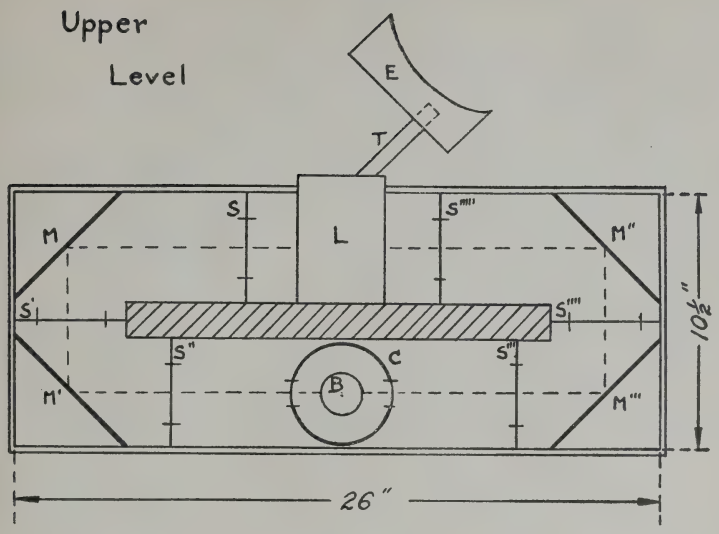


FIGURE 1
IMPROVED WOODWORTH PHOTOMETER
(Top View)

except for two round holes 1 inch in diameter in either side of the screen which permitted the rays to radiate in opposite directions. Six additional flat screens (*S*, *S'*, *S''*, etc.) with 2 1/2 inch holes shut off all rays reflected from the sides of the light passageway and permitted only straight-line light to reach the white reflecting surfaces.[4] This precaution guaranteed the accurate functioning of the inverse square law for the different light intensities.

The four 3 1/2 inch by 4 1/2 inch mirrors were made to order from the finest obtainable polished plate glass, which was beveled and silvered on the back. They were mounted on ball-joint braces for easy adjustment and in the finished photometer were framed with a one-half inch strip of black non-reflecting paper. This prevented refraction through the prismatic effect of the beveled edges.

The light passageway itself was 4 inches square except at the ends of the photometer box where the width was extended to 5 inches to permit adjustment of the mirrors. The floor of the light passageway was readily

[4]The path of the light is indicated by a broken line in Figure 1.

removable in small sections to offer access to the lower level of the box. A thin but strong brass upright moving through a narrow shielded slit in the bottom of the rear light passageway allowed the bulb and its circular screen to slide back and forth as the carriage in the lower level was moved, without admitting stray outside rays to the passageway.

The Lummer-Brodhun Photometer Cube (*L*) which received the light from each direction was a standard piece of photometric apparatus and consequently needs only a general description here. The comparison surfaces contained in this cube were of white plaster of Paris covered with magnesium oxide. Rays were reflected from these through a series of prisms and lenses and brought together in a focussable telescope (*T*) projecting at an angle from the front of the instrument. The field seen by the subject through this telescope was a circular one bisected vertically, subtending an angle of 6°. Each semicircular half contained within itself a smaller semicircle of light illuminated by rays from the opposite reflecting screen. The result was that rays seen in the left inner semicircle came from the same screen as those seen in the right outer simicircle, and vice versa. This system was designed to reduce retinal contrast to a minimum, and undoubtedly succeeded in doing so as well as any patterned field could.[5]

A black eyepiece (*E*) similar to that used in a stereoscope fitted tightly around the eyes of the subjects, effectively excluding side light. Although *Ss* were instructed to keep both eyes open within this eyepiece, only one eye was used in making judgments, a black screen covering the other. An adjustable chin-support, on which the head could be solidly rested, aided in the elimination of involuntary head movements.

2) *Lower Level.* The lower level of the photometer box (Figure 2) contained the sliding rods for the lamp carriage (*L.C.*) and the connections for the adjustable handles (*H*), used by the subject with the Method of Average Error, and (*H'*), used by the experimenter in all three methods. The lamp carriage consisted essentially in two short sections of steel tubing fastened solidly together by a rigid cross-piece. The carriage slid noiselessly and without play on parallel steel rods. The handles (*H* and *H'*) were of smooth brass, *S*'s handle (*H*) operating silently through four-to-one fiber reduction gears (*G* and *G'*).

Brass pulleys (*P* and *P'*) at either end of the lamp carriage supporting rods, carried a 1/16 inch flexible cable, which was kept taut by a small brass turnbuckle. The cable was hooked to each side of the lamp carriage at its center and in this way translated circular movements of the handles to linear movements of the lamp. An extension arm (*A*) of strong

[5]The Lummer-Brodhun Cube is probably the most accurate instrument available for comparing light intensities by the Equality of Brightness Method. Good observers using the present photometer with the cube set to its highest sensitivity (i.e., with contrast slips in place) could adjust the instrument to the point of objective equality within an error of no more than .12 of a meter candle.

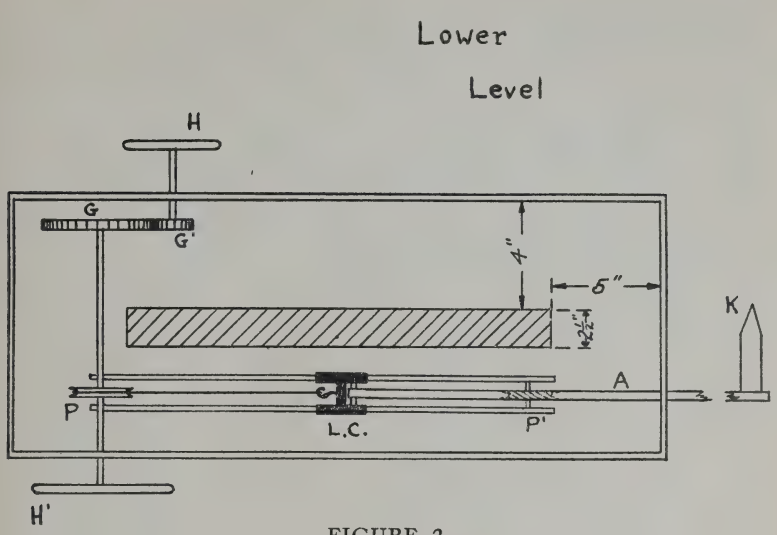


FIGURE 2

brass tubing 28 inches long was connected to the lamp carriage at its right side (as seen in Figure 2) immediately above the cable fastening. At the end of the arm a kymograph pointer ($K$) was attached. The arm was supported by a wooden pulley near the right wall of the box (not shown in figure), which rolled easily as the carriage was pushed back and forth. A Stoelting kymograph operating horizontally was used to obtain graphic records.

### B. Wiring

The source of current was a 6-volt, 100-ampere–hour storage battery. A voltmeter (Figure 3) shunted across the terminals of the photometer bulb gave a measure of the voltage used. The light circuit itself was a simple series circuit containing a 3-ohm variable resistance of radio type and a 3-ampere–range Weston ammeter which could be thrown in or out of the circuit by means of a double-throw single-pole switch. A revolving brass drum driven by an electric phonograph motor at reduced speed was also arranged by means of a shunt switch so that it could be thrown either in or out of the circuit. When the switch was closed the photometer lamp burned steadily.

The set-up was used with shunt switch closed for obtaining readings by the Method of Average Error or the Method of Minimal Change. With the Method of Constant Stimuli, however, the switch was open, and operation of the revolving drum caused the photometer lamp to flash on and off rhythmically, the duration of each illuminated period being 1

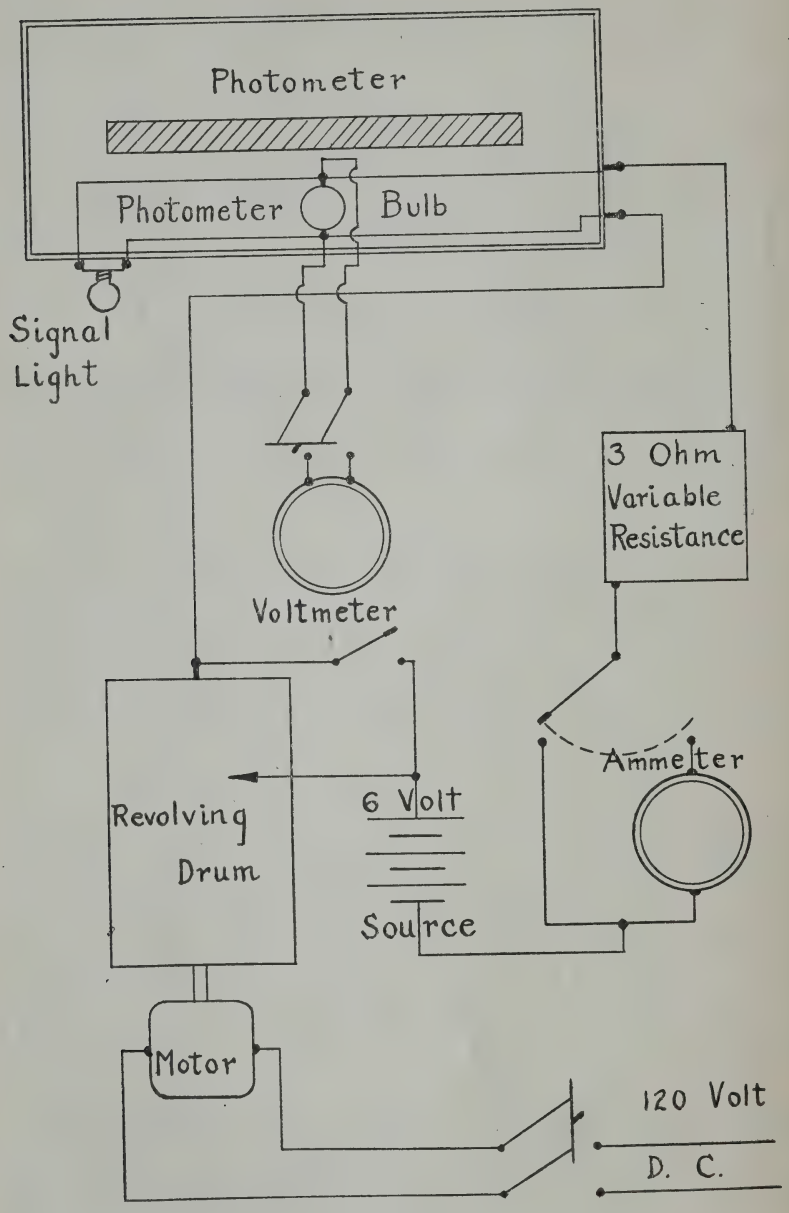


FIGURE 3
PLAN OF WIRING

second[6] and the dark interval between flashes, 2 seconds. The flashing was accomplished by breaking the circuit on the drum surface with a paraffin-paper stencil in which holes were cut at the proper distances. Because of the light-proof construction of the box, a small signal lamp was fastened outside near the handle ($H'$) and connected in parallel with the photometer bulb. In this way the sequence of flashes and dark intervals could be readily observed by the experimenter when the box was closed and the instrument in use.

C. Settings for Constant Stimulus Pairs

1) *Additional Apparatus.* For operation according to the Method of Constant Stimuli the following accessory parts were employed. Two small sheet metal arrows, one fastened about 3 mm. in front of the other, were rigidly attached to the photometer lamp carriage so that they moved back and forth with it over a half-millimeter–unit scale. About 4 mm. from their respective tips, which were painted black, each was pierced by a hole 1 mm. in diameter. The circumferences of the 1 mm. holes were painted white to make them stand out clearly from the darker surroundings. By closing one eye the experimenter could easily sight through these holes, as one would sight a gun. Head movements which might produce an angular line of vision were eliminated by the necessity of bringing the two super-imposed holes in line before the experimenter could see through them.

On a level with the line of sight and attached behind but protruding slightly above the reading edge of the scale were seven more arrows, painted white. The points of these were filed to a knife edge. They were firmly attached to a square brass rod behind the scale and could be slid along it as a means of varying the intervals between them.

2) *Method of Operation.* During the course of any particular experiment the seven white arrows were fastened by means of set-screws at definite points along the half-millimeter scale. They then functioned as indicators of the various empirically established positions at which the lamp carriage was placed for producing stimulus pairs of known difference.

Each flash of the light presented the subject with a pair of stimuli upon which a judgment was rendered. During the ensuing period of darkness,

[6]Bloch's law (*C. r. Soc. biol.,* 1885, 8th Series, Vol. II, 495) illustrates the necessity of keeping the duration of the periods of illumination constant. (Quoted by McDougall, W. The variation of intensity of visual sensation with the duration of the stimulus. *Brit. J. Psychol.,* 1904, **1**, 179.) "In the case of a light of brief duration and of such intensity as to be only just perceptible, the intensity must be varied inversely as the duration in order that the light may remain just perceptible." The case of lights of liminal intensity here cited is regarded by McDougall as a special limiting instance which lends support to the conclusion that the law holds for all intensities.

the experimenter set the lamp carriage to a point on the scale for the next comparison pair, by "sighting" the white arrow for that pair to the middle of the 1 mm. hole. A high degree of accuracy was attained by this technique.[7]

Preliminary tests to establish the optimum intervals between stimulus pairs gave results favoring a linear distance of 2.5 mm. With the middle arrow of the seven at the point of equality, successive stimulus pairs on either side produced per cent correct judgments in the neighborhood of 60, 80, and 95 respectively.

### D. Thresholds in Units of Illumination

1) *Calibration.* The photometer lamp was calibrated on a Standard Precision Photometer with a 50% sector. It was found to possess a candle-power rating of 10.1, when the voltage was 5 and the ammeter reading 2.35. In all experimental work the 3-ohm variable resistance was therefore adjusted to cut the potential from the maximum output of the battery to 5. This furnished an easy c-p. reading to use in calculations and proved a saving on both battery and lamp.

With the voltage held constant at 5, and the normal amperes for this voltage known, deviations in the internal resistance of the lamp could then be calculated by Ohm's Law.[8] If the ammeter reading shifted either above or below the 2.35 figure, it was significant of a change in the resistence of the filament and hence a change in the candle power. An accurate check was thus constantly available on the illumination produced, so that we knew whether fusion of the finely coiled filament of the bulb had distorted the calibrated readings.

The brightness center for the lamp, i.e., the point at which both of the white reflecting surfaces of the Lummer-Brodhun Cube were the same, was determined empirically from 150 adjustments made by 12 subjects.[9] A careful measure of the distance from the lamp filament when at the center position[10] to the plaster of Paris reflecting surfaces, revealed this distance to be 68.25 centimeters from each surface.[11] Applying the

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[7]In all probability the average error of the experimenter's settings was seldom more than one-tenth of a millimeter.

[8]In this case $R = \frac{E}{I}$

[9]This relatively small number of trials was sufficient because of the high degree of accuracy possible with the Lummer-Brodhun Cube when adjusted to finest sensitivity. In obtaining thresholds for experimental and not calibration purposes, however, the contrast slips were removed, thus reducing the sensitivity of the instrument and increasing the average errors.

[10]Bulb was placed in photometer with filament at right angles to the direction of movement.

[11]The brightness center therefore checked with the objective distance center.

Inverse Square Law, the illumination on each screen was calculated to be 21.682 lux.[12] Differences in meter candles between the illumination of the two screens, corresponding to deviations in millimeters from the center or zero position of the photometer lamp carriage were then determined. A curve plotted between these differences and the deviations was found to be linear well within the range of the thresholds (about 2 centimeters at a maximum on either side of the zero) and its equation was worked out to be

$$y = .1271\ x$$

Here $y$ is the deviation from zero *in millimeters* on the photometer scale reading, and $x$ is the difference between the illumination of the two white surfaces, or the *threshold value given by any particular reading in photometric units.* By the use of this equation single readings from the photometer scale, average threshold values, variabilities, or any other statistical constants calculated from the scale readings could be converted directly into lux, or if desired into millilamberts.[13]

As may be seen from the equation, lux values ($x$) can be figured to four decimal places. The mathematical precision here, however, is spurious to some extent, as the accuracy of the $y$ values used in the determination of $x$ is necessarily limited to *E*'s ability to make fine scale readings. However, with ordinary care and a large enough number of judgments to favor the cancellation of chance inaccuracies in the readings, calculated lux can be relied upon to the second decimal place.

2) *Thresholds.* Initial work with the apparatus places the general level of differential thresholds without Lummer-Brodhun contrast slips in place and with the light flashing intermittently, roughly between .32 and .64 meter candles; or expressed as a percentage of the brighter of the two lights compared, between 1.4 and 2.8 per cent. Thresholds obtained with light burning steadily are somewhat lower.

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[12]The standard photometric term for meter candles.

[13]The relationship for transmuting lux into brightness units or millilamberts has been succinctly stated by Woodworth as follows: "If the illuminated screen were a perfect diffuser of light, and also a perfect reflector ( . . . . . ) then its brightness (B) expressed in millilamberts would be numerically equal to 0.1 of its illumination (I) expressed in meter candles." Referring to the apparatus used by several investigators he says further, "Although in many cases the screens surely did not possess the properties thus assumed, it seems probable that the error so introduced is of less importance than those arising from other sources." Woodworth, R. S. Psychological data pertaining to errors of observation. *Internat. Critical Tables,* 1926, **1**, 92.

## A NOTE ON TONIC IMMOBILITY

Upon reading a paper by H. Hoagland, which the writer was good enough to send me, I find that in referring to some experiments of mine on the duration of the so-called death feint in *Ranatra* it is stated that "Holmes concluded that the animal learned to refrain from the futile death feint," and that "Holmes's evidence for 'learning' on the part of the animal apparently was due to the fact that he worked with only a part of one of these cycles." Since Mr. Hoagland has put the word learning in quotation marks, it might lead one to infer that it was quoted from my paper. I find on reading over the contribution referred to that I have neither used the word learning nor discussed the subject of learning at all. I am sure that Mr. Hoagland had no desire to misrepresent my position, but what led him to make me a defender of a view which I never thought of maintaining I cannot imagine. Elsewhere (*Studies in Animal Behavior,* page 212) I have suggested that on account of the muscular contraction involved in the so-called death feint "one would expect to find a diminution of the duration and perfection of the response as a simple consequence of fatigue." The inference that I worked with only a part of one of the death-feigning cycles is not correct, as I have often observed repetitions of the death-feigning cycle in the same individual.

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## CORRECTION OF FOREIGN ABSTRACTS OF "PRESENT-DAY TENDENCIES IN RUSSIAN PSYCHOLOGY" BY A. L. SCHNIERMANN[1]

### Les Tendances Actuelles de la Psychologie Russe

L'état actuel de la psychologie russe peut être caractérisé comme épanouissement des tendances matérialistes dans l'étude de la personnalité et du comportement.

Ces tendances dérivent de deux sources principales, dont la première est représentée par les anciennes doctrines objectives de Pavlov et Bekhterev. L'école de Pavlov, qui peut être caractérisée comme tendance strictement physiologique, a comme sujet fondamental l'étude objective de l'activité nerveuse superieure des animaux. La méthode principale de cette école est représentée par la méthode des réflexes conditionnés, basés principalement sur les réflexes salivaires innés. L'école réflexologique de Bekhterev a pour but l'étude objective et bio-sociologique de la personnalité en ses manifestations extérieures. Parmi les méthodes principales de cette école, qui permettent de s'approcher à l'analyse physiologique du comportement,

[1] *J. Gen. Psychol.,* **1**, 397-404.

il faut citer la méthode de réflexes associés (le mécanisme des réflexes "associés" est identique au celui des réflexes conditionnels; cependant l'école de Bekhterev admet comme base, principalement, les réflexes défensifs (moteurs). Outre l'analyse physiologique, l'école de Bekhterev inserre dans son programme l'étude génétique du comportement humain; elle escompte aussi des facteurs socials.

La seconde source est représentée par les nouvelles tendances matérialistes psychologiques. Parmi ces dernières c'est l'école matérialiste de Kornilov, grandi sur les ruines de l'ancienne école idéaliste de Tchelpanov, qui exerce la plus grande influence. Basé sur le matérialisme dialectique, Kornilov vient à la conclusion, que la seule et unique méthode scientifique, qui puisse établir une cohérence causale entre les phénomènes, c'est la méthode objective; la méthode de l'introspection sans contrôle de données objectives, perd toute valeur. Cependant Kornilov admet, qu'on puisse employer la méthode d'introspection en forme de "témoignage verbal," à côté des méthodes objectives.

Les autres tendances objectives (par example l'ecole de Bassov) exploitent l'introspection encore d'avantage, que l'école de Kornilov. Les essais de ces écoles en fait de l'unification des méthodes objectives et subjectives, amènent à l'affaiblissement du travail analytique, ayant cependant une grande accumulation du matérial empirique. Parmi les autres doctrines psychologiques, qui réunissent les méthodes subjectives et objectives, il faut citer l'ancienne école de "psychologie comparative," de Wagner. Cette école, qui étudie les problêmes psychologiques au point de vue de l'évolution, exploite plusieurs méthodes pour les diverses phases du développement psychique, aussi l'analyse du comportement, contient-elle grâce à l'emploi des méthodes subjectives, encore quelques élements téléologiques.

Il faut constater, que presque toutes les écoles psychologiques russes ont une certaine tendance objective et matérialiste.

### Gegenwärtige Tendenzen der Russischen Psychologie

Der gegenwärtige Zustand der Psychologie in S.S.S.R. kann als ein Aufblühen der materialistischen Strömungen in der Lehre von der Persönlichkeit und ihrem Betragen charakterisiert werden.

Diese Strömungen entspringen zwei Grundquellen: die erste wird durch die alten, von Pawlov und Bechterew begründeten objektiven Richtungen in der Lehre vom Betragen repräsentiert. Der Grundinhalt der Pawlowschen Schule, die als strikt physiologische Richtung charakterisiert werden kann, besteht in der objektiven Erforschung der höhern Nerventätigkeit der Tiere. Die Grundmethode ihrer Arbeit ist die der "bedingten" Reflexe; letztere werden hauptsächlich auf Grund angeborener Speichelreflexe gebildet. Die reflexologische Schule Bechterews hat sich die objektive biosoziologische Erforschung der menschlichen Persönlichkeit in ihren

äussern Kundgebungen zum Ziel gesetzt. Ihre Grundmethode zur physiologischen Analyse des Betragens ist die der "Assoziationsreflexe" (der Mechanismus der "Assoziationsreflexe" ist mit dem der "bedingten" Reflexe identisch, doch werden bei den Arbeiten der Bechterewschen Schule hauptsächlich die motorischen Defensivreflexe als Grund angenommen). Ausser der physiologischen Analyse nimmt die Schule Bechterews das genetische Studium des menschlichen Betragens und auch die sozialen Faktoren in ihr Programm auf.

Die zweite Quelle wird durch die neuen materialistischen psychologischen Richtungen repräsentiert. Den grössten Einfluss unter ihnen hat die materialistische Schule Kornilows, die aus den Trümmern der alten idealistischen Schule Tschelpanows emporgewachsen ist. Sich auf die Basis des dialektischen Materialismus stützend, gelangt Kornilow zum Schluss, dass die objektive Methode allein als wissenschaftliche Methode, die die kausale Verbindung der Erscheinungen aufdeckt, angesehn werden kann; die Methode der Introspektion ohne Kontrolle der objektiven Daten verliert jegliche Bedeutung. Doch hält Kornilow es immerhin für möglich die introspektive Methode in Form eines Rechenschaftsberichts neben den objektiven Methoden anzuwenden. Die andern "objektiven" psychologischen Richtungen (z.B. die Schule Bassows) benutzen die Introspektion in einem noch grösseren Masse als die Schule Kornilows. Die Versuche die objektive und die subjektive Methode zu vereinigen, führen zur Schwächung der analytischen Arbeit bei grosser Anhäufung des empirischen Materials.

Unter den andern, sowohl subjektive, als auch objektive Elemente vereinigenden psychologischen Richtungen muss die alte Schule der "vergleichenden Psychologie," geleitet von Wagner, genannt werden. Diese Schule, die vom evolutionell-genetischen Standpunkt aus ans Studium der psychischen Prozesse herangeht, benutzt beim Studium der verschiedenen Entwickelungsstufen der Psyche verschiedene Methoden. Hierbei enthält noch die Analyse des Betragens dank der Benutzung der subjektiven Methode teleologische Elemente.

Überhaupt lässt sich feststellen, dass fast in allen russischen psychologischen Schulen eine bestimmte objektiv-materialistische Tendenz bemerkbar ist.

## СОВРЕМЕННЫЕ ТЕЧЕНИЯ В РУССКОЙ ПСИХОЛОГИИ

Современное состояние психологии в СССР может быть охарактеризовано, как расцвет материалистических течений в учении о личности и ее поведении. Эти течения происходят из двух основных источников: первым являются старые об'ективные направления в учении о поведении, основанные Павловым и Бехтеревым. Школа Павлова, которая может быть охарактеризована как чисто-физиологическое направление, имеет своим основным содержанием об'ективное изучение высшей нервной деятельности животных. Основным методом ее работы является метод „условных" рефлексов, образуемых, главным образом, на основе врожденных слюнных рефлексов. Рефлексологическая школа Бехтерева имеет целью об'ективное био-социологическое изучение человеческой личности в ее внешних проявлениях. Одним из основных методов, позволяющих подойти к физиологическому анализу поведения, является в этой школе метод „сочетательных" рефлексов (механизм „сочетательных" рефлексов идентичен с механизмом „условных" рефлексов; однако, в работах Бехтеревской школы за основу принимаются, главным образом, двигательные оборонительные рефлексы). Кроме физиологического анализа, школа Бехтерева включает в свою программу генетическое изучение поведения человека, а также учет социальных факторов.

Вторым источником являются новые материалистические психологические направления. Наибольшим влиянием среди них пользуется материалистическая школа Корнилова, выросшая на руинах старой идеалистической школы Челпанова. Стоя на основе диалектического материализма, Корнилов приходит к заключению, что единственно научным методом, вскрывающим причинную связь явлений, может быть только метод об'ективный; метод же интроспекции без контроля об'ективных данных теряет всякое значение. Однако, Корнилов все же считает возможным применять интроспективный метод в форме „словесного отчета"—на ряду с об'ективными методами. Другие „об'ективные" психологические направления (примером их может служить школа Басова) пользуются интроспекцией еще в большей мере, чем школа Корнилова. Попытки этих школ об'единить об'ективный и суб'ективный метод приводят к ослаблению аналитической работы при большом накоплении эмпирического материала.

Среди других психологических направлений, соединяющих суб'ективный и об'ективный подход, следует отметить старую школу „сравнительной психологии" возглавляемую Вагнером. Эта школа, подходящая к изучению психических процессов с эволюционно-генетической точки зрения, пользуется различными методами при изучении различных ступеней развития психики. При этом анализ поведения, благодаря пользованию суб'ективным методом, еще содержит некоторые телеологические элементы.

В общем, следует признать, что почти во всех русских психологических школах наблюдается определенный сдвиг в сторону об'ективного материалистического подхода.

**Шнирман** (Schniermann)

# BOOKS

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HUNTER, W. S. *Human Behavior.* Chicago: Univ. Chicago Press, 1928. Pp. x+355. $2.00.

In this text for elementary psychology Professor Hunter adopts a behavioristic point of view. He maintains that the term psychology is no longer applicable to much of what is being studied by psychologists. "Anthroponomy" is suggested as a substitute for the term psychology. He says, "No one denies that the world is composed of colors, tones, stones, trees, and other similar objects; but no one has shown either that these objects are psychic processes or that the understanding of human nature would be advanced through their analysis. Such sciences as physics, chemistry, and biology are quite competent to describe and explain man's environment. Human nature can best be understood when it is made the direct object of study, and not when it is approached through the medium of the analysis of the world into tastes, smells, colors, pressures, and configurations" (p. v.).

In the introductory chapter Hunter justifies his point of approach by the statement, "The behaviorist has studied so many forms of behavior under so many different conditions that he is warranted in believing that behavior results only from present stimuli and from the retained effects of previous behavior plus whatever influence hereditary conditions may exercise" (p. 6). As to the difference between traditional psychology and anthroponomy, the author states that anthroponomical explanations are largely genetic involving to a large extent accounts of the previous training of the subjects as well as attempted descriptions of inherited probabilities of response. To this genetic explanation of a given phenomenon, neurological data are added where they are available (p. 9).

The text proper begins with short descriptions of man and his place in the world, the difference between man and the animals, the antiquity of man illustrated with a phylogenetic ancestry chart from Osborn, man and his place in the world, invention of the scientific method, the continuity of development of man's behavior.

Chapter I is called phylogenetic anthroponomy, and starts with a brief description of man's genetic relationship to the remainder of the animal kingdom and shows the continuity of sensory, motor, and central activities, throughout the phylogenetic series. There is a brief introductory description of the conditioned-reflex method of habit formation as based upon the neural properties of sensitivity, motility, conductivity. The symbolic reaction is held to be an essentially human reaction and is derived from the delayed response. A symbol is defined as ". . . any process which is a substitute, which can arouse a selective response, and which can be recalled if it ceases to be present" (p. 67).

The next chapter deals with the classification of individual behavior on the basis of tests. There is a description of the testing method and the way it is applied for establishing the capacity of the individual in relation to other individuals. As a basis for the chapter on abnormal behavior, Hunter gives the universal principle ". . . that each individual is in constant conflict with other individuals and with various parts of his own individuality" (p. 102). To the reviewer the term *conflict* is not well chosen. From the standpoint of science there is no conflict. There is merely a change from one act to another, each change being the resultant of the antecedent external and internal conditions. In conclusion we find, "The chief personality defect which stands out as a result of the studies of abnormal behavior is *dissociation*. Typically this involves an inability to bring the behavior which is relevant to bear upon the problem at hand" (p. 122).

Under the chapter on social and racial anthroponomy we find the topics: the origin of society, the place of instincts in social life, social uniformities, imitation, suggestion, social institutions, the nature of custom, the mob, and racial differences. While each of the topics is treated more scientifically than is usually done in the sociological texts which deal with the same subjects, yet even Hunter has done little more than present the usual literary descriptions.

In Part II the author takes up normal adult anthroponomy. The first chapter is mainly devoted to neurology. In the second and third chapters the unlearned or reflex behavior is treated. "A reflex act is a simple inherited mode of response controlled by the nervous system" (p. 175). The topics of instinct, and experimental studies in the modification of instinct, the relation between inheritance and environment are clearly presented. Hunter questions the value of making a distinction between instinct and emotion. He regards an emotion as the visceral or skeletal components of low intensity which are merely a part of the instinctive reaction. Other topics in these chapters are: the visceral components in instinctive behavior, the influence of total visceral disturbance upon overt instinctive behavior, facial behavior as instinct, etc.

The next two chapters deal with the receptor processes and include

much of the material usually given under sensation and perception in the traditional text.

For the reviewer, Hunter's real contribution begins with Chapter VI on Habit. The best criterion for the different psychological systems is the way in which the subject of habit-formation is treated and it is from his treatment of this topic that Hunter gets his behavioristic classification. He defines habit as, ". . . an acquired specific form of verbal or non-verbal response such as writing, reading, running the maze, or piano playing. In each case the response is aroused only when some stimulus or combination of stimuli acts upon the receptors. Each acquired response is a co-ordination of inherited and acquired forms of response" (p. 275). As topics we find: retention, methodology, transfer of training, nature of instruction, whole vs. part method, setting of habits, capacity for habit-formation, rate of forgetting, learning. The fixation of habit arcs is regarded as the essential factor in learning. To indicate how this is done the theories of Angell, Thorndike, Carr, and Watson are presented. According to Hunter the important factors are frequency, recency, and the facilitating and inhibiting effects of avoiding reactions and visceral disturbances (p. 295). A more detailed treatment of facilitation and inhibition would have improved the presentation.

In contrasting instincts and habits we find that instincts serve to adjust the organism to its environment in ways that have proved not too disadvantageous in the past history of the species. Likewise habits serve to adjust one to his environment in ways that have proved not too disadvantageous in its own lifetime. Each automatism enables the individual to bring the results of past training to bear upon the present problems (p. 299).

The selective factor in habit-formation is regarded as movement. We read, "Movement, change, is the fundamental characteristic of the stimulus through which the control of behavior by that stimulus is established" (p. 304). The mechanism of habit reinstatement is illustrated in Figure 73.

A distinct contribution is made in the section which deals with the equivalence of stimuli and responses. "Two stimuli are equivalent when they will arouse the same response. One response is equivalent to another when it produces the same result as that other" (p. 322). This leads to the definition ". . . where two responses result in equivalent stimuli, the responses themselves are equivalent" (p. 323). This distinction makes it clear how two stimuli or responses may be physically different but alike socially.

The last chapter is on thinking. It brings together many of the principles outlined in the preceding chapters. At the basis of thinking is language. "Verbal and gestural behavior are processes (*a*) which can be reinstated in the absence of the original stimulus that called them forth, (*b*) which function in the place of other less abbreviated forms of response,

(*c*) which can determine one of two or more externally possible forms of behavior, and (*d*) which have a high social utility in the field of inter-stimulation and response" (p. 327). As a definition we find, "Thinking is a sequence of responses directed toward the solution of a problem where the problem has been formulated by the subject's own language responses and where this formulation partially controls the sequence" (p. 335).

On the very controversial topic of purpose, we learn that ". . . the only way in which a goal can influence the preceding behavior is through the facilitating and inhibiting influence of instructions, which in their turn are reinstated language responses" (p. 336). As a final statement of the value of science we find that "Because infrahuman animals lack language processes in any significant degree, they have never formulated cause and effect relationships, and hence have been unable to regulate their behavior with reference to this aspect of the world" (p. 342).

An excellent list of references is given at the end of each chapter. The illustrations are numerous and, on the whole, good. But the diagrams on neural functions cannot be regarded as adding much to the clearness of the presentation. If the reviewer were using the book as a text, he would begin with Part II instead of Part I. While there is some advantage in beginning with the more familiar (although more complicated) conditions of everyday life, this necessarily anthropomorphic approach makes it difficult to shift to the objective conception of science for which the book was written. However, after all is said, the reviewer believes that Hunter's book is one of the best recent elementary texts in psychology. It conforms much more to the topics found in the traditional treatment of psychology than do Meyer's and Watson's, and at the same time these topics are handled as objectively as our present state of knowledge will permit.

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HARTSHORNE, H., & MAY, M. A. *Studies in Deceit.* New York: Macmillan, 1928. Bk. I, pp. xxi+414; Bk. II, pp. viii+306. $4.50.

That the experimental method is rapidly taking over the fields of social psychology and of ethics is evident from this remarkable investigation of deceptive behavior by Hartshorne and May, already favorably known for their experimental work on character. The authors have stressed methodology as their primary consideration, and rightly so at the present stage of inquiry. Ethical training, as they see it, has suffered from the lack of an experimental basis and the methods in vogue have been as likely to damage character as to improve it.

Deceit, as a subject of investigation, is a significant choice, since society

is honey-combed with it. The authors, however, are concerned with a purely objective study of this aspect of human nature; they made no attempt to evaluate motives or to raise the question of the rightness or wrongness of a particular act. Nor have they attempted to survey the whole range of deceptive conduct; they have limited themselves to one small corner of the field, namely, cheating in ordinary classroom situations, in school athletic contests and social parties, and in schoolwork done at home. Two lying tests and two stealing tests have been included in their battery.

Narrowly conceived, their investigation is an attempt to apply the current methods of tests and measurements to a new field. They were, therefore, confronted with the usual tasks incident to psychological measurement. It was necessary (1) to devise and standardize techniques suitable for determining objectively whether or not deceptive behavior has occurred in a controlled situation; (2) to determine upon the specific situations to be utilized and the populations to be tested; and (3) to select test material and to standardize the conditions under which it should be given and the methods of scoring results. Each phase of their task involved statistical considerations.

In devising a suitable method for testing deceit, the authors were able to profit to some extent by the work of earlier investigators, notably Cady and Voelker. They have also utilized earlier published results of their own. As finally evolved and standardized, three techniques were employed: (*a*) The Duplicating Technique, which consisted in the illegitimate use by the pupil of a key or answer sheet in doing or scoring his work under conditions which made it possible for the invesigators to identify such behavior; (*b*) The Improbable Achievement Technique, where achievement above a certain level indicated deception, as, for example, in Coordination Tests carried out with eyes closed in which cheating involves opening the eyes to improve one's score; (*c*) The Double Testing Technique, where pupils were tested twice, once with a strict supervision and no opportunity to cheat, once with full opportunity to do so. The difference between the scores so found is used as a measure of the tendency to deceive.

The situations utilized include work done at home and in the classroom, athletic contests, games at social parties. Various groups of children, mostly from fifth to the eighth grades, were chosen in such a way as to have samples from different social, economic, and cultural levels, various intelligence levels, different types of community of unlike socialization and character (delinquents excepted), and of various national, racial, occupational and religious composition. Both sexes were tested.

The test material included intelligence tests developed by the Institute of Educational Research (the IER tests), various psychological speed tests, tests of physical capacity, puzzle tests, coordination tests. For testing

stealing, a planted dime test was used and a magic square puzzle involving the use of money. Two questionnaires were employed to test lying. The first was specific in content, "Did you ever cheat on any sort of test?" and the like. In this case the invesigator knew the correct answer. The answers to the second questionnaire were motivated by a desire to win approval by claiming possession of improbable virtues.

In treating the results statistically the Xi score was used; this is the "actual deviation from the honest mean divided by the standard deviation of the honest differences."

The tests were applied to some 11,000 children and the effect on deceit of the following factors was estimated: age, sex, intelligence, emotional stability, physical condition, cultural background, nationality, religious affiliations, kinship, grade retardation, school status and morale, suggestibility, and movie attendance. The authors summarize their findings as follows:

"The concomitants of deceit are in order of their importance, (1) classroom association; (2) general personal handicaps, such as relatively low IQ, poor resistance to suggestion, and emotional instability; (3) cultural and social limitations in the home background; (4) other loosely correlated miscellaneous facts."

Apart from their specific findings the authors reach a number of general conclusions that are significant from a theoretical point of view, for example, the dismissal of the virtues as pyschological entities. "They are not acts. They are classifications of acts." Honesty is not to be conceived as a unified trait. An individual honest in one specific situation may be dishonest in another. His degree of consistency is a function of the situations in which he is placed, whether or not they possess identical elements, whether he has learned to be honest or dishonest in them and become aware of their consequences. Such a conception of virtue parallels one of the two present-day conceptions of intelligence. It carries, moreover, definite implications relative to moral training. To quote the authors, "If there is anything in the school which puts a premium on subterfuge, it would be folly to imagine that any teaching of honesty whether in school or out would greatly alter the actual practice of the children. Hand in hand with the development of codes must go the types of experience in which honor is the natural and rewarded behavior to be learned by practice in those situations where the child finds himself ethically at home."

The ordinary school drive for school success, measured by high marks and promotion from grade to grade, is a big factor in motivating deceptive behavior if the resistance is low. The child of low IQ may have recourse to deceit to fulfil the demands made upon him, though sometimes as in some of the speed tests he is not clever enough to cheat and so is surpassed in amount of cheating by the more intelligent children. Progres-

sive school methods were found to be accompanied by a decrease in the amount of deception. On the other hand, one organization which purported to teach honesty showed a positive correlation between dishonesty and length of membership in it.

To the reviewer, one of the most interesting chapters of the book was a study of the characteristics of three outstanding groups, the extremely Honest, the extremely Dishonest, and the Confessors, or those children who in the questionnaire investigation acqnowledged their deceit. As a group the Confessors possess an unusually large number of personal handicaps, more even than the Dishonest group. The latter are particularly weak in family relationships. But in spite of striking group differences it is not possible to determine why particular children are honest or dishonest. There is a considerable over-lapping of handicap scores, the it is not possible to determine why particular children are honest or dishonest children of few handicaps. Such cases serve to emphasize the need of individual as well as mass studies.

It is difficult to do justice to a volume so full of implications both in the way of method and of theory. There are many hints in the book as to possibilities in the way of investigating group behavior, the very thing needed to make social psychology into an experimental science. An example of this sort is found in the study of class morale. Striking differences between class groups appeared in the amount of cheating and these differences were maintained. Evidently classroom associations were very important. The teacher-pupil relation was also important, though only an occasional individual teacher exerted a marked influence. The teachers were simply part of a system of sequent groups, each such group accumulating from year to year its characteristic habits.

The statistical handling of so great amount of material as that covered by the volume being reviewed presented many problems. They are discussed in detail in Book II, where the expert in such matters will find the material he needs for estimating the success of the venture from this angle. The reviewer who is no expert must leave such evaluation to others. The reliability of the tests appears to be high. The tests are held to be valid as measures of the specific situations with which they deal. They are records of a specific mode of response in a specific situation and claim to give no knowledge of a unified trait. An adequate treatment would require a comprehensive sampling of all situations which evoke deceptive behavior. The present study gives a picture of characteristic classroom attitudes and suggests that the fact of deception is an instance of unfortunate maladjustment and a symptom of underlying conditions that need remedying.

The appendix contains material that will enable anyone who desires to do so to duplicate any phase of the authors' investigation. The

latter foresee that publicity will destroy much of the usefulness of their techniques.

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BENTLEY, M.; DUNLAP, K.; HUNTER, W. S.; KOFFKA, K.; KÖHLER, W.; MCDOUGALL, W.; PRINCE, M.; WATSON, J. B.; & WOODWORTH, R. S. *Psychologies of 1925: Powell Lectures in Psychological Theory.* (Ed. by Carl Murchison.) Worcester, Mass.: Clark University Press, 1926. Pp. 412. $6.00.

It is rather good fun to be reviewing a book two years after the date of its publication and three years after its material was first delivered to the public. Most particularly is this the case when the book has a title with such transitory suggestions as *Psychologies of 1925.* Almost am I persuaded that I am a philosopher, viewing at least one thing *sub specie aeternitatis.* It is fair to say that the delay is my fault and not the editor's, who very kindly gave me this chance to say what I think about these lectures a long time ago. But after all, I gather that the book still maintains itself strongly and is running a rapid course through a third edition. In fact, when the *Psychologies of 1930* or the *Psychologies of 1935* come to be published, I would not be surprised if they find it very difficult to depose these old-fashioned utterances from their well-deserved public regard.

I have done what I suppose no reviewer ought to do, and read a lot of other notices of this book. Most of them have been appreciative, and with those I heartily agree. They save me the duty of writing a number of superlatives. They also mean that I can select for comment some contributions that have so far been given, perhaps, less consideration than they deserve. My doing this must not be taken to mean that I rate them more highly than the rest, but only that I do not wish to write what other people have already written well. I should, for example, like to have written all the nice things I have read about the contributions to this book of Professor Kurt Koffka and Professor Wolfgang Köhler. They are the sort of lectures that make you think "Why didn't I say that first?", but that make you think it with admiration untinged by envy so admirably are they expressed. Koffka's discourse upon what he takes to be the genuine units of psychological science is beautifully clear, and if I suspect that some of the general admiration for Köhler's second lecture, "An Aspect of Gestalt Psychology," is due to a feeling that what you can't understand must be important, that is perhaps only because I am in the same box. I can't for the life of me see what there is in *Gestalttheorie* which makes it necessary to show elaborate parallels between phenomeno-

logical aspects of perception and physiological processes. And I think that Köhler's physical speculations about the interior of the brain is the sort of biophysics that would make the real biophysicist sit up and gasp. But nobody can deny that this is a brave utterance, though it may be running ahead a few hundred years. Similarly, I am sure I should have delighted to hear Watson, in his best staccato manner, demolishing McDougall and everybody else; and McDougall, in measured and comely phrase, demolishing Watson. As I read the three long and fundamental lectures of Morton Prince, I see again a well-known lecture room here in Cambridge, England, and a large crowd of medical students listening delightedly to such a racy and human exposition of basic biological notions as only Dr. Prince could give; for some of the notions find a more public and permanent place in these pages. Of them, and of many of the other lectures, I should like to try to write critically and at length, for it is small compliment for a man to skim lightly over the ideas which represent his most serious views of the science of human life. But I refrain, for space is real and a matter of most earnest concern to an editor, and others have written in detail about these lectures already.

I shall turn to only two sets of lectures which I read myself with the most lively interest and which, for some reason or other, seem to have escaped much notice in most of the reviews of the book that I have seen.

The first is Professor W. S. Hunter's lecture, "Psychology and Anthroponomy." How the audience must have nejoyed it! How they must have wondered every now and then whether he would really get his tongue properly round this new and fearsome word! No doubt he did, for a father will suffer a lot for the honor of his own infant. Anthroponomy is not so bad, but what about the inevitable derivatives, anthroponomical, anthroponometry, and anthroponometrical, and so on. Frankly, my conservatism is in revolt, and I don't like it. But what is it all about?

The argument is rather long and decidedly complicated, but it seems to amount to this. Both psychology and anthroponomy study man in certain of his "phases." But psychology can do this indirectly only, because it must try to get at man through his environment and not through his reactions. This lands it into a lot of psychologically, I suppose I should say anthroponomically, fruitless discussion as to whether the environment is physical, or mental, or both, or neither. Anyway, in some manner or another, psychologists of all sorts persist in interposing consciousness or experience between man and whatever extra-conscious, or experiential, environment he may be supposed to have, and this obscures both that environment and man himself. The anthroponomist, however, has a clearer vision. All his problems concern the relation of reaction to stimulus. He studies directly the whole organism of man. The bulk of the reactions that he is concerned with are, no doubt, language reactions. These may perhaps be described as conscious reactions, and this description may

perhaps be true. But true or false it contributes nothing to the solution of any scientific question. We can dismiss from our school those naughty boys "experience" and "consciousness," because they only muddle us with epistemological problems and never do a stroke to help us to understand the complex life of man.

Well, I find it pretty nearly incredible that there should have been all this bother about consciousness for all these years if the solution were as simple as that. I think it demonstrable that into the very stimulus-reaction series with which Hunter is concerned there enters consciousness, or at least there enter facts of a conscious order. These do not merely exhaust themselves in being aware. They have something to do. They are causal factors in a stimulus-reaction series. The demonstration has yet clearly to be made. But I would take a good big bet that it will be made some day. And then the psychologist, working with the exact problem that Hunter has formulated, will not need to forego what Kai Lung might call his "honourable title."

Meanwhile, every psychologist who takes his work seriously ought to read and ponder this good lecture. It focusses an essential problem. It challenges us to show that when we say "so and so is a fact of consciousness" we mean that the fact of its being conscious veritably makes a difference to our modes of reaction. It is unfair, I think, in some of its destructive criticism, in its treatment of meaning according to the Wundtian tradition, in some of its statements about *Gestalttheorie,* particularly in its persistent refusal to admit that a psychologist can possibly remain loyal to a biological approach to the life of man. But for the way it warns us against wandering in epistemological labyrinths, for its healthy scorn of speculative physiology, for its downright and emphatic statement of what scientific psychology must aspire to do, it is a contribution of permanent interest and value.

Another sort of lecture to which I turned with eagerness was the three concluding discourses by Professor Madison Bentley on what is usually called "structural" psychology. For structural psychology is at present very much under a cloud, and I hoped that this distinguished author of these lectures would set up such a wind as might blow the cloud away. But I must confess to considerable disappointment. For Professor Bentley finds that the standpoint of the structuralist "represents a closed chapter in psychological history." He gives an extremely interesting, clear, and, as I think, accurate account of what they did. He, rather apologetically, enumerates some of their virtues, and he joins the general chorus to point out their defects. But there is no really vigorous defense of the notion of mental structure, and no clear discussion of whether or not a psychologist can afford to dispense with this notion. Yet it seems as if many of the responses with which the psychologist is directly concerned must have material with which to work; and that even if we adopt a biological

point of view, the qualitative characteristics and the mode of organization of this material—images, ideas, trains of reasoning, and the like—must be described if we are to understand how they influence the responses with which they are related. Perhaps this is what Professor Bentley means when he says that "experience is to be considered as an integral part of the organism, and it is to be described in such subdivisions or aspects as its own nature suggests and to be treated as a part of the activity of the total organism." But this statement seems to me very vague and unsatisfying. I want to know just how he would work it out in reference to some specific problem or other. And for that I, at least, would willingly do without the final lecture on "The Psychological Organism" in which the lecturer seems to strive to produce a sort of Garden of Eden in which not only the *Psychologies of 1925* but all the psychologies of all time will lie down in peace together.

When the reader has finished this book and recovered from the flash and glitter of all the brilliant general ideas, he may wonder whether there is a science of psychology, different from any of these sectional psychologies, slowly building up. He may be tempted to believe that there is, and that it is being built, less upon the public platform and by the controversies of the schools, and far more by the patient and laborious work of the physical and physiological laboratories, by the investigations of the biochemist, the pathologist, the neurologist, and the zoölogist, by the observations of the naturalist and the medical clinician. He may even—but this would appear a most heretical view—believe that it is growing slowly out of the work of the experimental psychological laboratory.

But all this will not dim his enjoyment of the *Psychologies of 1925,* nor, it is to be hoped, of its successors in the same series.

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American Psychiatric Association. Proceedings of the first colloquium on personality investigation. Baltimore: Lord Baltimore Press, 1929. Pp. 102. Paper $.60; cloth $1.00.

Baar, J. Psychology. New York: Globe, 1928. Pp. vii+126.

Bode, H. B. Conflicting psychologies of learning. Boston: Heath, 1929. Pp. vi+305. $2.00.

Chassell, J. O. The experience variables. Rochester: Author, 1928.

Edgerton, H. A., & Toops, H. A. Academic progress: a follow-up study of the freshmen entering the University in 1923. *O. St. Univ. Stud.: Contrib. Adminis.,* No. 1. 1929. Pp. x+150.

Elwell, F. H., & Fowlkes, J. G. Elwell-Fowlkes bookkeeping test. Yonkers, N. Y.: World Book, 1929. $1.30. Specimen set $.30.

Fletcher, H. Speech and hearing. New York: Van Nostrand, 1929. Pp. xv+331. $5.50.

FRANKLIN, S. P. Measurement of the comprehension difficulty of the precepts and parables of Jesus. *Univ. Ia. Stud.: Stud. Educ.*, 1928, No. 155. Pp. 63. $1.00.

FREEMAN, F. N. Mental tests. Boston: Houghton Mifflin, 1926. Pp. ix+502.

KANTOR, J. R. An outline of social psychology. Chicago: Follett, 1929. Pp. xiv+420.

KELLEY, F. M., RUCH, G. M., & TERMAN, L. M. New Stanford achievement test. Forms V, X, Y, and Z. Yonkers, N. Y.: World Book, 1929.

KÖHLER, W. Gestalt psychology. New York: Liveright, 1929. Pp. x+403. $4.00.

LADD-FRANKLIN, C. Colour and colour theories. London: Kegan Paul; New York: Harcourt, Brace, 1929. Pp. xv+287. $3.50.

MARSTON, W. M. Emotions of normal people. New York: Harcourt, Brace; London: Kegan Paul, 1928. Pp. xii+405.

MILES, W. R., & STARCH, D. [ED.] Seashore commemorative number. *Psychol. Monog., Univ. Ia. Stud. Psychol.*, 1928, **34**. Pp. 223.

MÜLLER-FREIENFELS, R. Die Hauptrichtungen der gegenwärtigen Psychologie. Leipzig: Quelle & Meyer, 1929. Pp. 135. M 1.80.

PACK, E. P., *et al.* Research in the social sciences. New York: Macmillan, 1929. Pp. xii+305.

PILLSBURY, W. B. The history of psychology. New York: Norton, 1929. Pp. 326.

PINTNER, R. Educational psychology. New York: Holt, 1929. Pp. xiv+378. $2.50.

RUSSELL, B. Our knowledge of the external world. New York: Norton, 1929. Pp. ix+268. $3.00.

SAMPSON, C. M. A practice of physiotherapy. St. Louis: Mosby, 1926. Pp. 620.

SCHEIDEMANN, N. W. Experiments in general psychology. Chicago: Univ. Chicago Press, 1929. Pp. xiii+108. $1.00.

SCHNEERSON, F. Studies in psycho-expedition. New York: Brown, 1929. Pp. xiv+209. $3.50.

SEASHORE, C. E. The present status of research in the psychology of music at the University of Iowa. *Univ. Ia. Stud.: Stud. Educ.*, 1928, No. 157. Pp. 29.

SNOW, A. J. Effective selling. 3 vols. Chicago: Shaw, 1929. Pp. 380.

STANTON, H. M. Prognosis of musical achievement. Rochester, N. Y.: Univ. Rochester, Eastman School of Music: *Stud. Psychol.*, 1929, **1**. Pp. 89.

WEISS, A. P. A theoretical basis of human behavior. Columbus, O.: Adams, 1929. Pp. xviii+480.

ZOETHOUT, W. D. Laboratory experiments in physiology. St. Louis, Mo.: Mosby, 1928. Pp. 252. $2.25.

————. Textbook of physiology. St. Louis, Mo.: Mosby, 1928. Pp. 264. $4.50.

# FUNCTIONAL DISTURBANCES OF HEARING IN GUINEA PIGS AFTER LONG EXPOSURE TO AN INTENSE TONE*

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## I

The field of auditory theory has long been a fruitful one for the minds of ingenious and imaginative scientists, and the great number of theories that have been formulated since Helmholtz first outlined his resonance theory stand as evidence of a great and abiding general interest. The ear is essentially mechanical in its action, and this probably gives us a clue as to why so many people should find unfailing interest in theorizing upon the functioning of its complex mechanism, while the eye, for instance, being in its action essentially chemical, should claim the attention of such a relatively smaller group of theorists.

New principles and new facts are continually appearing from the laboratories, demanding reformulation of existing theories or resulting in the establishment of new ones. Those who hold to an established theory stand always ready to seize upon any new facts to bolster further their position and to tear away the supports of their opponents. When the results of Yoshii's experiment (17) were first published in 1909, they were immediately seized upon as clear support for the Helmholtz view, and practically all statements of the "place theory" since that time have cited his work in support of it. Out of consideration of the results of Yoshii's experiment and of their value for auditory theory, the present experiment arose.

It has long been known that extended exposure to intense noise results in auditory disturbances, and the deafness of boiler-makers who have spent considerable time at that trade serves as a proverbial example. Habermann (5, 6) successfully established a relation be-

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tween occupational exposure to noise resulting in boiler-makers' deafness and degeneration in the cochlea. The changes which he found consisted of atrophy of part of the organ of Corti as well as atrophy of the corresponding nerve fibers. Habermann concluded that the action of intense prolonged sound is predominantly and primarily on the sense cells proper (cells of Corti), and that, as a consequence of this injury, atrophy progressively develops in the cochlear nerves. Most of the cochleas examined showed changes which were confined to the lower part of the base, and Habermann stated that most of the tones and noises occurring in the working environment of smiths are of high pitch and great intensity. Rodger (11), who has investigated cases of occupational deafness more recently, is in substantial agreement with Habermann. He concludes that the locus of the initial lesion in the inner ear of steel-workers is determined by the predominant noises which exist in their environment, and that depreciation of hearing is confined to the pitches to which the noises correspond. Habermann stated that the noises were of very high pitch, while Rodger places them between $A_1$ (410 cycles) and $C_2$ (512 cycles).

Wittmaack (16) carried on experimentation in which animals were exposed to intense tones, and examination of the cochleas showed gross changes in the cochlear mechanism. His results derived from cases of experimentally produced degeneration in animals are in perfect accord with those of Habermann, and demonstrate that the destruction in such cases is confined to a definite strip of the cochlear canal.

Yoshii recognized the importance of Wittmaack's experiment, and set out to repeat it, using pure tones. He exposed guinea pigs to continuous tones for periods of from 30 to 100 days. Four different tones ($c^5$, $c^3$, $b^2$, and $g$) were used with different groups of animals exposed to each tone. After exposure the ears were examined histologically. Local transverse degenerations were found in the organ of Corti, extending over a range of one-quarter to one-half turn of the cochlea. These degenerations of the auditory mechanism occurred in the order of the frequencies of the tones, with the highest frequency nearest the base of the cochlea. The regions of disturbance were broad, and not sharply defined, with the greatest change in the middle of the affected region, decreasing above and below. The extent of the change varied with the intensity of the tone, the greatest change being found in the region affected by the most intense tone.

Yoshii deduces from his results clear support for the Helmholtz theory.

"Die Ergebnisse der experimentellen Untersuchungen mit reinen Klängen geben uns weiter eine neue wichtige Stütze für die wohl bekannte aber bisher noch nicht völlig bewiesene Resonanztheorie von Helmholtz. Es ist besonders zu berücksichtigen, dass bei jedem Versuche entsprechend der Höhe der angewandten Pfeifen die Schneckenskala nur an einer und zwar ganz charakteristischen Streke geschädigt wurde: Bei dem Versuche mit einer Pfeife $c^5$ wurde konstant die obere Hälfte der Basalwindung geschädigt, bei dem Versuche mit $h^2$ kam die Alteration eine halbe bis eine ganze Windung, mit g 1½ Windungen höher oben zu liegen. Je höher also die Tonquelle ist, um so tiefer liegt die lädierte Stelle der Schneckenskala" (17 p. 244).

It is not generally agreed that these findings stand unambiguously in support of the Helmholtz theory, which regards the transverse fibers of the basilar membrane as resonators that are set into motion by the sound waves corresponding to their natural periods of vibration. It remained for Boring (2) to show that the evidence from physiological degeneration also serves quite adequately as support for the frequency notion, which regards the basilar membrane as a vibrating unit which responds, in whole or in part, in a way depending upon the intensity of the sound. A very intense sound would on this view disturb a relatively large part of the basilar membrane, while a less intense sound would disturb a relatively shorter section beginning at the base of the cochlea and reaching upward.

"If a stimulus continues to displace the organ of Corti many times a second for two months, it is not at all improbable that degeneration might occur at the point where the moving region joins the quiescent region, for at this point the greatest strains would be produced. It seems to me, therefore, probable that Yoshii got destruction for $c^5$ much nearer the base of the cochlea than the destruction for g because the amplitude of $c^5$ was much less than the amplitude for g, and not at all because the small end of the basilar membrane lies at the base of the cochlea" (2, p. 184).

If Yoshii's results are to be used in support of the Helmholtz theory, we must assume that the amplitude of the tone bore no relation to the locus of the degeneration correlated with exposure to the tone. The degenerated region would be that region affected only by the particular tone to which the animal was exposed. Whatever the amplitude might be, the same general region would always be affected by the same tone, and if the degeneration were complete we would expect the animal to be unable to hear the tone. If the

frequency theory is the correct one, it would seem sensible to assume that, although destruction was observable in the organ of Corti, it would be, as Boring has pointed out, at the juncture of the moving section of the basilar membrane with the quiescent portion and would not interfere to any great extent with the animal's reception of the tone. It was to test this point that the present experiment was designed.

Yoshii's work demonstrates quite clearly that certain definite changes in the auditory mechanism do follow long exposure to intense tones, but without functional differentiation his results must occupy an equivocal position as regards the two types of theories. Since further structural differentiation appears to be impossible at present, it would seem that definite knowledge of the nature of the *functional* disturbances following long exposure to intense tones should have general significance for auditory theory.

By a previous experiment it was demonstrated that relatively pure tones were effective as stimuli in the environment of guinea pigs (14). It was proposed in the present experiment to expose guinea pigs for an extended period to a tone of high intensity and of the same pitch as the conditioning tone used in the earlier experiment. The exposure was to be followed by an attempt to condition the exposed animals to the original conditioning tone and to another tone equal to the conditioning tone in intensity but differing considerably in pitch. If the animals could be reconditioned to the original conditioning tone, and destruction of part of the basilar membrane could reasonably be expected in the light of other researches (16, 17), then the logical deduction would be that the degenerated section was not necessary for the reception of the conditioning tone. If, on the other hand, it should be found impossible to recondition the animals, we could assume that degeneration had occurred and that the degenerated section had originally served as the local mediating mechanism for the conditioning tone. Any general influence of the exposure tone on the auditory mechanism would probably appear in the results of an attempt to condition an animal to another tone differing in pitch from the exposure tone but equal to the original conditioning tone in intensity.

## II

The apparatus used in this experiment was identical with that used in the experiment upon conditioning. The Bosch loud speaker

and the General Radio (7) type 213 oscillator[2] of 600 cycles, driven by a four-cell Edison storage battery (5.2 volts) served as the source of the exposure tone.

The highest intensity available with the 600-cycle oscillator ($600i_3$) was used as the exposure tone, because a destructive intensity was desired. No way of stating the relative intensities of tones is known to the writer, so the tone used here cannot be compared to the tones used by Yoshii. Human observers reported pain in the ears after exposure to the tone for periods varying with different observers from five to ten minutes.

The center of the loud-speaker diaphragm was five feet six inches from the floor, and a piece of felt one inch thick separated it from the supporting structure. During the exposure period the animals were kept in two cages, which were placed two feet and eight feet, respectively, from the center of the loud speaker. The near cage was somewhat lower than the plane of the far cage and the loud speaker, so as not to obstruct the line between the far cage and the source of the tone. This distribution into two cages was made so that each group would be affected by a different intensity of the exposure tone. The tone was operated continuously for 70 days, except for a 30-minute period each day. Quiet during this period was required in another experiment which was being carried on in an adjoining room. The battery was changed regularly and often, to insure a constant energy output from the oscillator. Four animals were kept in the near cage and eight in the far cage. Two of the animals in each cage had been conditioned to respond to $600i_1$. The ages of the animals ranged between 10 and 14 months. As soon as the exposure period was terminated, all animals were tested for direct response to the three available intensities of both the 600- and the 1000-cycle oscillators. The apparatus was the same as that used to condition the first four animals to the $600i_1$ stimulus (14).

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[2]The type 213 oscillator is equipped with four terminals from secondary coil. The first one is the common lead, and the other three vary in energy output giving three discrete intensities.

A type 213 oscillator of 1000 cycles was used later in the experiment. The energies delivered at corresponding terminals of the 600- and 1000-cycle oscillators were equal according to a factory test. To avoid confusion the frequencies and intensities of the tones used will be designated by the following symbols: $600i_1$, $600i_2$, $600i_3$, $1000i_1$, $1000i_2$, and $1000i_3$; $600i_1$ was the conditioning tone used in the earlier experiment and $600i_3$ was the exposure tone in the present experiment; $600i_2$ and $1000i_2$ represent the intermediate intensities.

Four animals that had been kept out of the experimental situation until this time were introduced as controls, making three separate groups: the far-cage group, which had been under the influence of the exposure tone at a distance of 96 inches; the near-cage group, which had been under the action of the exposure tone at a distance of 24 inches; and the control group. All animals were tested in the sound-proof box with a distance of 24 inches separating the experimental animal from the source of the tone. This distance was measured from a point on top of the head, midway between the ears, to the center of the loud-speaker cone. The animals of the near-cage group were further tested with but six inches separating them from the source of the tone to determine the relative magnitudes of the responses under conditions such that the ratios of the test intensity to the exposure intensity would be the same for each group. The results of these tests can be best shown in tabular form (see Table 1).

The data in this table indicate that an increase of sensitivity, followed by a decrease in sensitivity to a particular frequency, is correlated with the temporal course of long exposure to the tone. The three groups can be viewed as representing three points on a temporal continuum. The control group represents the state of the auditory

TABLE 1

R—Response in 24-inch position
O—No response in 24-inch position
r—Response in six-inch position
o—No response in six-inch position

| Group | Intensity | Frequency 600 cycles $600i_1$ | $600i_2$ | $600i_3$ | 1000 cycles $1000i_1$ | $1000i_2$ | $1000i_3$ |
|---|---|---|---|---|---|---|---|
| Control | 1 | O | R | R | O | R | R |
| group | 2 | O | R | R | O | O | R |
| | 3 | O | O | R | O | R | R |
| | 4 | O | R | R | O | R | R |
| Far- | 1 | R | R | R | O | R | R |
| cage | 2 | R | R | R | O | R | R |
| group | 3 | R | R | R | O | R | R |
| | 4 | R | R | R | O | R | R |
| | 5 | R | R | R | O | O | R |
| | 6 | R | R | R | O | R | R |
| | 7 | R | R | R | O | O | R |
| | 8 | R | R | R | O | O | R |
| Near | 1 | O o | O o | O o | O | O | R |
| cage | 2 | O o | O o | O o | O | R | R |
| group | 3 | O o | O o | O r | O | R | R |
| | 4 | O o | O o | O r | O | O | R |

mechanism before the exposure, and I believe that it can safely be assumed that the state of the auditory mechanisms of the members of the near- and far-cage groups was the same as that of the control group before they had been subjected to the exposure tone. The two exposure groups were exposed for the same length of time, but the intensities of the tone at the two cages were different, since the intensity of a tone varies inversely as the square of the distance separating the source of the tone and the place where it is heard. It would seem sensible to assume that the effect of the exposure tone would be greatest where the intensity was greatest, thus placing the near-cage group nearer to or beyond the point in development where total destruction occurs.

If the sensitivity of the control group is taken as normal, significant deviations from the normal are to be seen in the data from the two exposure groups. The intensity limen for the control group falls somewhere between $600i_1$ and $600i_2$, with none of the animals responding to $600i_1$ and three of them definitely responding to $600i_2$. All intensities of the 600 frequency elicited responses from all members of the far-cage group, indicating an intensity limen below $600i_1$. None of the intensities from the 600-cycle oscillator, with a 24-inch separation from the animals, elicited responses from any of the animals in the near-cage group. This indicates a limen somewhere above $600i_3$. When a distance of six inches separated the animals from the source of the tone, two of the animals gave very small responses to the highest intensity. This would indicate that the intensity limen for this group was far above the highest intensity obtainable with a 24-inch separation between the animal and the source of the tone, since the intensity of the tone as apprehended in the six-inch position was some sixteen times as great as the apprehended intensity in the 24-inch position.

The results of the tests with the 1000-cycle oscillator seem to indicate that no general changes were brought about by exposure to the highest intensity of the 600-cycle oscillator. The responses of the animals of all three groups to the three intensities of the 1000-cycle oscillator, and of the animals in the control group to the three intensities of the 600-cycle oscillator (in the 24-inch position) were uniformly similar; no significant differences are discoverable in these four sets of data. If the conditions of the cochleas of the control animals were normal, and I have no reason to believe that they were not, and if the data from the exposure groups are comparable

with those of the control group for a particular tone, the logical deduction must be that the mechanisms mediating the apprehension of that particular tone in the cases of the animals of the exposure groups were normal.

The data as recorded in Table 1 reveal no differences in the *magnitudes* of the responses to the different frequencies and intensities. A comparison of the responses of the animals to the different tones was possible because the method of experimentation afforded opportunity for the collection of permanent objective records of the magnitudes of significant responses, by means of the recorded breathing movements, and of the relations of these responses to the significant stimuli.

The magnitudes of the responses varied with the frequency and intensity. By determining the ratio of the magnitude of each response cycle in the breathing curve to the magnitude of the normal breathing cycle, the necessary data for a graphic representation (see Figure 1) of the temporal course of the changes in the auditory mechanism were obtained. The magnitude of the normal breathing cycle in the case of each response was determined by measuring the heights of the ten cycles preceding the response cycle in the breathing curve and averaging these ten values. This ratio,

$$\frac{\text{Magnitude of response}}{\text{Magnitude of normal breathing}} = X$$

was determined for all intensities of both frequencies (600 cycles and 1000 cycles). There were five presentations of each intensity of both frequencies. The five values for each intensity were averaged to give a general value for the response of each animal to each intensity of each frequency. The homogeneous data in each group were averaged, giving a general value of the group response to each intensity of each frequency. A value of 1 was assigned to those cases where no response accompanied presentation of a tone, since the cycles in the breathing curve coincidental with the tone were roughly but nearly enough equal to the ten cycles preceding the tone. The values of the group responses are represented in Table 2. The occurrence of the figure 1 in the table indicates that there was no response.

The question arose here of the possibility of conditioning the two animals from the near-cage group that had previously been

conditioned to $600i_1$. The apparatus and conditions were identical with those in the earlier successful attempt at conditioning (14). This attempt was unsuccessful, no evidence of conditioning being recorded after *1580* tone-shock presentations in the case of one animal and *1700* in the case of the other. In the light of the results of the earlier experiment, in which clear evidence of the apprehension of tones by the animals occurred in the breathing records after ap-

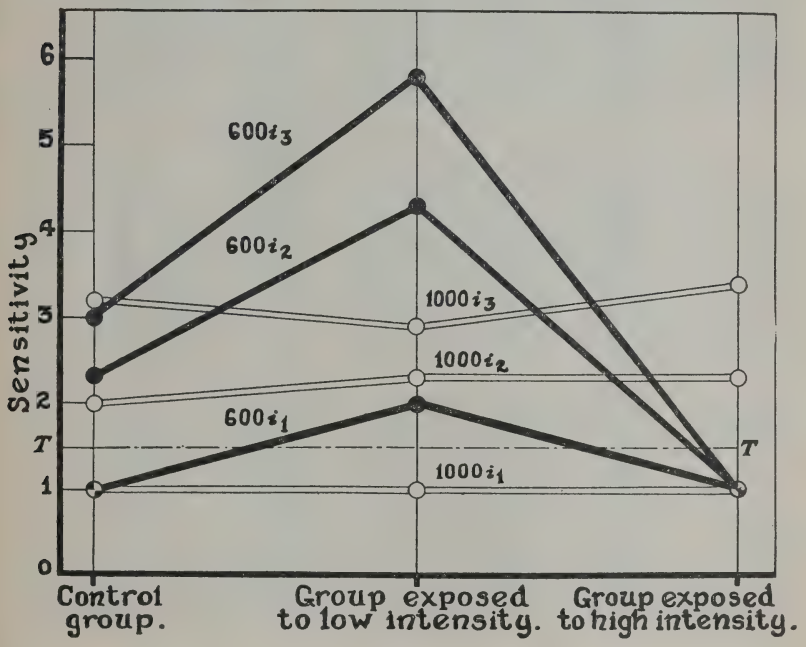


FIGURE 1

The values recorded in Table 2 which represent the sensitivity of the three groups to the three intensities of the two frequencies are plotted on the abscissa. The three points on the ordinate represent the three groups of animals. The control group had not been exposed to any tone. The other two groups were exposed to a tone with a frequency of 600 cycles. One of these groups, the far-cage group, was 96 inches from the source of the tone, and the other, the near-cage group, was 24 inches from the source of the tone during the period of exposure. Therefore the intensity of the exposure tone differed greatly for the two groups. The solid lines represent the three intensities of the exposure fequency (600 cycles). The open lines represent the three intensities of the test frequency (1000 cycles). *T-T* represents the hypothetical threshold of response to tones presented directly to the animal in the absence of any unconditioned stimulus.

TABLE 2

$F_0$—Frequency. G—Group. $i$—Intensity.
C—Control Group. F—Far-Cage Group.
N—Near-Cage Group.

| $F_0$ | 600 cycles | | | 1000 cycles | | |
|---|---|---|---|---|---|---|
| G | C | F | N | C | F | N |
| $i$ | | | | | | |
| $i_1$ | 1 | 2 | 1 | 1 | 1 | 1 |
| $i_2$ | 2.3 | 4.3 | 1 | 2 | 2.3 | 2.3 |
| $i_3$ | 3.0 | 5.8 | 1 | 3.2 | 2.9 | 3.4 |

proximately 250 tone-stock presentations, with a true conditioned response at about the 500 tone-shock presentation, it would seem that some definite change had taken place in the auditory mechanisms of the two animals. That the change was not general is demonstrated by the fact that the responses of these animals to tones with a frequency of 1000 cycles were of the same general nature and magnitude as the responses of animals known to be normal. Since Yoshii demonstrated that destruction of sections of the basilar membrane accompanied exposure to intense tones, and since it would appear from the results of the present experiment that after long exposure to a tone of sufficiently high intensity, lower intensities of the same frequency are inaudible to the animal so exposed, it seems safe to presume that destruction of that part of the basilar membrane mediating apprehension of the inaudible intensities had taken place.

## Summary of Results

1) Exposure to an intense tone over an extended period of time is accompanied by the appearance of definite functional changes in responses of guinea pigs to auditory stimuli:

*a*) There is a total loss of sensitivity to tones of the exposure frequency within a very wide range of intensities. A low degree of sensitivity to very high intensities of the exposure frequency remains after the exposure period.

*b*) When the exposure tone is of lower intensity, an increase of sensitivity to the exposure frequency takes place.

2) Exposure tone does not apparently affect the sensitivity of the exposed animals to other frequencies.

## III

Inferences from results of experimentation upon guinea pigs as

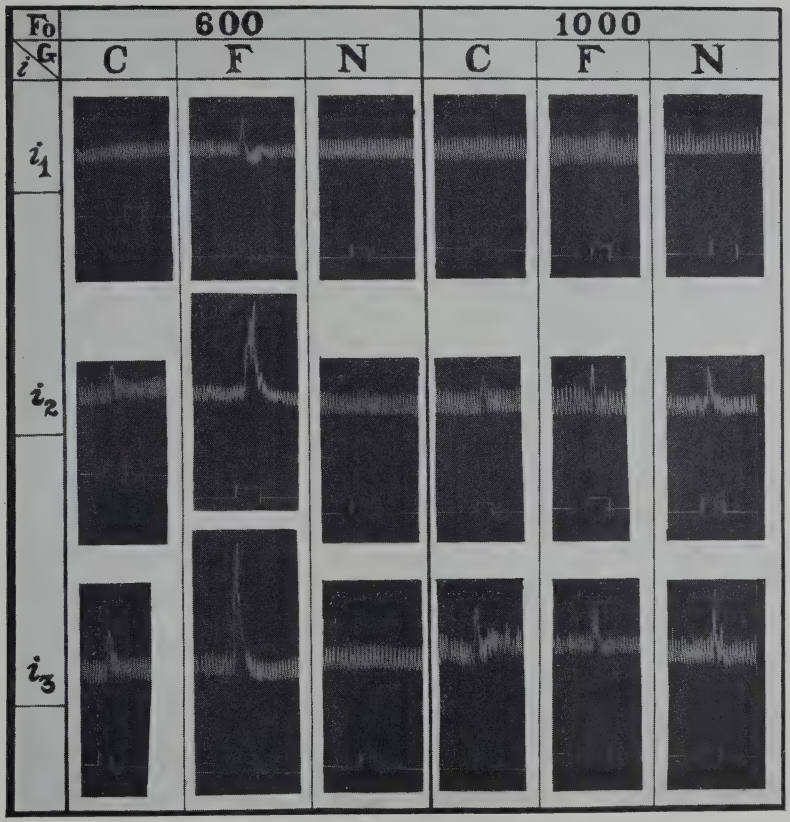


FIGURE 2

SAMPLES OF THE RECORDS OF RESPONSES TO THE DIFFERENT INTENSITIES AND FREQUENCIES

$F_0$—frequency. *G*—group. *C*—control group. *F*—far-cage group. *N*—near-cage group. $i_1$, $i_2$, $i_3$, represent the three intensities of both frequencies, $i_1$ being the lowest intensity, $i_3$ the highest, and $i_2$ the intermediate intensity.

Careful adjustment of the straps which held the pneumograph in place on the animal resulted in a smooth regular breathing record. If the straps were adjusted too tightly the breathing was very irregular, resulting in wide variation in the amplitude of the respiration cycles as recorded in the breathing curve.

concerns general auditory theory are to be made with a clear realization of the difficulties involved. Auditory theories are, in general, based upon facts relating to the human ear and to experience which is mediated by the human auditory mechanism. There are, however, limits to the kinds of experiment that can be carried on with human subjects. For experimental material in investigations for which human subjects cannot be used, one naturally turns to some other mammal in which a structure similar to the human ear is to be found, the universal and pragmatically justified practice being to infer similarity of function from similarity of structure. It is exactly the same procedure under which one or a dozen human cochleas are assumed to be similar to human cochleas in general.

The guinea pig was chosen in this instance for several reasons: first, because it has been used in other experiments, the results of which I wished to consider in the light of my own experiment; secondly, because the ear of this animal has been used widely in histological work and has been found to be remarkably similar in structure to the human ear (13, pp. 884-893; 1, pp. 480-498; 3, pp. 220-224; 10, pp. 1510-1523; 12, pp. 301-333).

It was pointed out in the introductory section that the evidence obtained from the results of Yoshii's experiment can be used in support of either the place or frequency theories of the perception of sound. Yoshii showed that prolonged exposure to an intense tone results in damage to the organ of Corti. The damage is localized in transverse strips occurring in the order of the frequencies of the exposure tones with the highest frequency nearest the base of the cochlea. Since both the frequency and the amplitude of the tones which Yoshii used varied, the damage to the organ of Corti might have been a function of either one. If frequency determined the locus of the damage the evidence is for a place theory, but if amplitude determined the locus of the damage the evidence supports a frequency theory.

The writer believes that the present experiment furnishes evidence which clears Yoshii's results of this ambiguity. It demonstrates that long exposure to an intense tone results in a total loss of sensitivity to tones of the exposure frequency within a very wide range of intensities. This loss of sensitivity was shown by the failure of an attempt to condition the animals to a tone of low intensity of the exposure frequency and by their inability to respond to intensities of the exposure frequency which invariably elicited responses from normal animals.

In view of the facts cited above, the writer is constrained to decide in favor of a place-pitch or resonance theory of hearing. Such a theory would regard the cochlea as an organ of pitch discrimination. This discrimination would depend upon vibration of specifically localized transverse strips of the organ of Corti which would be set into vibration by the sound waves corresponding to their natural periods of vibration. Thus pitch is taken care of, but a theory of hearing must also account for the perception of intensity. Yoshii found that the width of the damaged strips of the organ of Corti varied with the amplitude of the exposure tones, with the widest strip of damage correlated with the tone of greatest amplitude. This linear spread over the basilar membrane, with its center at the fiber whose natural period of vibration corresponds to the stimulating frequency, offers an explanation of the perception of intensity (15, pp. 154-158; 4). Each increment of energy would result in an increase of amplitude of the stimulus and therefore a wider spread along the basilar membrane, thus including additional nerve fibers. The number of fibers stimulated would, upon this assumption, be a direct correlate of the intensity of the perceived tone. It is interesting to note that Hecht's (8, 9) hypothesis of the perception of visual intensity embraces a similar idea. It is assumed in this hypothesis that the individual receptors in the retina have different thresholds of sensitivity to light. A light of low energy stimulates only a certain number of the receptors within the area of the retina upon which it impinges. As the energy of the stimulating light increases, receptors of higher threshold values are stimulated, and the intensity of the perceived light is a correlate of the number of receptors stimulated.

## Summary

1) Exposure to an intense tone over an extended period of time is accompanied by the appearance of definite functional changes in the responses of guinea pigs to auditory stimuli:

*a*) There is a total loss of sensitivity to tones of the exposure frequency within a very wide range of intensities. A low degree of sensitivity to very high intensities of the exposure frequency remains after the period of exposure.

*b*) When the exposure tone is of lower intensity, an increase of sensitivity to the exposure frequency takes place.

2) Exposure to an intense tone does not apparently affect the sensitivity of the exposed animals to other frequencies.

3) The facts established by the experiment support a place-pitch or resonance theory of the perception of sound which regards the cochlea as the organ of pitch discrimination.

4) An explanation of the perception of intensity is offered by the linear spread of the disturbance set up in the basilar membrane by a stimulating sound, thus making the number of fibers stimulated a direct correlate of the intensity of the perceived tone.

## References


1. Böhm, A. A., Davidoff, M., & Huber, G. C. A textbook of histology. Philadelphia: Saunders, 1910.
2. Boring, E. G. Auditory theory with special reference to intensity, volume, and localization. *Amer. J. Psychol.*, 1926, **37**, 157-188.
3. Dahlgren, U., & Kepner, W. A. A textbook of the principles of animal histology. New York: Macmillan, 1921.
4. Gray, A. A. A restatement of the resonance theory of hearing. *Acta Oto-Laryngol.*, 1927, **11**, 30-53.
5. Habermann, J. Beiträge zur Lehre von der professionallen Schwerhörigkeit. *Arch. f. Ohrenhk.*, 1906, **69**, 106-130.
6. ————. Ueber die Schwerhörigheit der Kesselschmeide. *Arch. f. Ohrenhk.*, 1890, **30**, 1-25.
7. Halverson, H. M. The audio-oscillator. *Amer. J. Psychol.*, 1927, **38**, 294-295.
8. Hecht, S. A quantitative basis for the relation between visual acuity and illumination. *Proc. Nat. Acad. Sci.*, 1927, **13**, 569-574.
9. ————. The relation between visual acuity and illumination. *J. Gen. Physiol.*, 1928, **11**, 255-281.
10. Piersol, G. A. Human anatomy. Philadelphia: Lippincott, 1919. Pp. 2104.
11. Rodger, R. The pathological effects of excessive sounds on the cochlear apparatus considered in relation to the theories of sound perception. *J. Laryngol. & Otol.*, 1923, **38**, 66-71.
12. Schäfer, E. A., & Symington, J. Quain's elements of anatomy. Vol. III. New York: Longmans, Green, 1909.
13. Schneider, K. C. Lehrbuch der vergleichenden Histologie der Tiere. Jena: Fischer, 1902. Pp. 988.
14. Upton, M. Auditory sensitivity of guinea pigs. *Amer. J. Psychol.*, 1929, **41**, 412-421.
15. Wilkinson, G., & Gray, A. A. The mechanism of the cochlea. London: Macmillan, 1924. Pp. xx+253.
16. Wittmaack, K. Schädigung des Gehörs durch Schalleinwirkung. *Zsch. f. Ohrenhk.*, 1907, **54**, 37-62.
17. Yoshii, U. Experimentelle Untersuchungen über die Schädigung des Gehororgans durch Schalleinwirkung. *Zsch. f. Ohrenhk.*, 1909, **58**, 201-250.


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## LES DÉSORDRES FONCTIONNELS DE L'AUDITION CHEZ LES COBAYES EXPOSÉS PENDANT LONGTEMPS À UN TON INTENSE

(Résumé)

En 1909 Yoshii publia les résultats d'une expérience où il montra que des changements de structure d'une nature spécifique accompagnaient un sujet exposé pendant longtemps à des tons intenses. On a accepté partout ces résultats comme bonne évidence pour la théorie de Helmholtz. Boring a montré récemment que les résultats de Yoshii peuvent soutenir aussi bien une théorie de "fréquence." On a désigné cette expérience-ci pour déterminer la nature des changements fonctionnels qui accompagnent un sujet exposé pendant longtemps à des stimuli tonals intenses.

On a exposé deux groupes de cobayes à un ton intense avec une fréquence de 600 cycles pendant une période de 70 jours. Les distances séparant les deux groupes de la source du ton ont été différentes pour que chaque groupe soient soumis à une intensité différente. A la fin de la période on a testé les animaux pour des réponses directes à plusieurs intensités de la fréquence de 600 cycles. Les animaux exposés à la basse intensité ont montré plus de sensitivité comparés avec des animaux normaux tandis que ceux exposés à la haute intensité ont été insensitifs aux tons de la fréquence de 600 cycles. Des tests avec une série de tons avec une fréquence de 1000 cycles ont indiqué que les changements sont spécifiques pour les tons avec une fréquence de 600 cycles. On a essayé sans succès de conditionner à un ton de 600 cycles les animaux exposés à la haute intensité. Les résultats de cette expérience soutiennent une théorie "place pitch" de l'audition.

UPTON

## FUNKTIONELLE STÖRUNGEN DES GEHÖRS BEI MEERSCHWEINCHEN NACH LÄNGERER EINWIRKUNG INTENSIVER TÖNE

(Referat)

Im Jahre 1909 veröffentlichte Yoshii die Ergebnisse eines Experiments die Bewiesen dass die längere Einwirkung starker Töne gewisse spezifische Strukturveränderungen verursacht. Diese Resultate wurden allgemein als deutlicher Beweis für die Helmholtzsche Theorie angesehen. Boring hat kürzlich dargetan dass Yoshiis Resultate ebensowohl zur Bestätigung einer Häufigkeitstheorie dienen können. Das vorliegende Experiment hatte den Zweck die Art der funktionellen Veränderungen zu bestimmen, die durch die längere Einwirkung von starken Töne verursacht werden.

Zwei Gruppen von Meerschweinchen wurden 70 Tage lang den intensiven Tönen—mit einer Häufigkeit von 600 Cyklen—ausgesetzt. Die Entfernung der beiden Gruppen von der Tonquelle war nicht die gleiche, so dass die Wirkung auf jede Gruppe nicht von gleicher Stärke war. Am Ende der Exponierung prüfte man die Tiere mit Tönen von verschiedener Intensität. Die Meerschweinchen welche Tönen von geringer Intensität ausgesetzt worden waren, zeigten erhöhte Empfindlichkeit im Vergleich zu normalen Tieren, wogegen jene, welche Tönen von starker Intensität ausgesetzt worden waren, unempfindlich blieben gegen die Töne der Exponierungs-häufig-

keit. Tests mit einer Serie von Tönen mit der Häufigkeit von 1000 Cyklen deuteten an, dass die Veränderungen spezifisch für die Häufigkeit von 600 Cyklen waren. Ein Versuch die Tiere, welche man Tönen hoher Intensität ausgesetzt hatte, an einen Ton von 600 Cyklen zu gewöhnen blieb ohne Erfolg. Die Ergebnisse dieses Experiments unterstützen die Theorie von "place pitch" (Stellentonhöhe) des Gehörs.

UPTON

## ФУНКЦИОНАЛЬНЫЕ НАРУШЕНИЯ СЛУХА У ГВИНЕЙСКИХ СВИНОК ПОСЛЕ ДЛИТЕЛЬНОГО ВОЗДЕЙСТВИЯ ИНТЕНСИВНОГО ЗВУКА.

(Реферат).

В 1909 г. Иошии (Joshii) опубликовал результаты опыта, при помощи которого было показано, что длительное воздействие интенсивными звуками сопровождается структурными изменениями специфической природы. Эти результаты получили широкое распространение как несомненные данные в пользу теории Гельмгольца. Боринг (Boring) недавно показал, что результаты Иошии являются как раз хорошим подкреплением теории „частот" („frequency theory"). Поставленный им опыт имел целью определить природу функциональных изменений, сопровождающих длительное воздействие интенсивных звуковых раздражителей.

Две группы гвинейских свинок подвергались в течение 70 дней раздражению интенсивным звуком с частотой в 600 колебаний. Расстояния, отделявшие обе группы от источника звука, были различны, вследствие чего каждая группа подвергалась воздействию звуков различной интенсивности. По окончании периода воздействия животные были подвергнуты испытанию с целью выяснить, не образуются ли у них прямые реакции на раздражения звуками различных интенсивностей и частот. Животные, подвергавшиеся действию звуков низкой интенсивности, обнаруживали по сравнению с нормальными животными повышенную чувствительность, в то время как животные, подвергавшиеся действию звуков высокой интенсивности, были нечувствительны к звукам с пред'явленной частотой колебаний. Испытанные на серию звуков с частотой в 1000 колебаний, обнаружили, что изменения являлись специфичными для звуков с частотой в 600 колебаний. Испытание животных, которые были подвергнуты действию звуков высокой интенсивности, на звуки с частотой в 600 колебаний осталось безуспешным. Результаты этого опыта подкрепляют теорию „определенной настроенности" слуха.

**Эптон (Upton).**

# STUDIES OF EMOTIONAL REACTIONS
# VI. CARDIAC RESPONSES*[1]

*From the Pathological Laboratory of Maudsley Hospital, London*

CARNEY LANDIS AND DAVID SLIGHT[2]

As part of the response to a sudden, unexpected ("surprise") stimulus, frequent reference is made to apparent changes in heart action, which are described in varied terms, one of the most common being that the heart has missed a beat.

In earlier studies of this series (5, 6), it was stated that "surprise" is the only one of the so-called emotional feelings for which evidence of pattern response was found. It was demonstrated that in response to sudden, unexpected stimuli there occurs a sharp rise in blood-pressure followed by an immediate fall. The work of Robbins (7) indicates that sudden stimulation leads to vasoconstriction followed by a slower vasodilation process. Blatz (1) found that a sudden, unexpected, backward fall produced a very rapid increase in pulse rate, followed by a return to normal within one-half minute; and then came a secondary acceleration with a further return to an approximate normal in about half a minute. These findings show that sudden stimulation brings about very rapid circulatory changes.

## PROBLEM

The object of this study was to determine what changes in the electrocardiogram, both as to form and time relations, would result from sudden stimulation likely to cause upset of a so-called emotional nature in normal subjects and in those suffering from

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[2]The writers wish to express their profound appreciation to Dr. F. Golla, of the Pathological Laboratory, Maudsley Hospital, London, for his guidance, suggestions, and helpful criticisms of this work, also to Dr. Mapother, of Maudsley Hospital, for his kindness in allowing us to make use of the hospital patients. This work was carried on during Mr. Landis' tenure as a National Research Fellow in the Biological Sciences and Dr. Slight's tenure as a staff member of the Maudsley Hospital.

certain psychopathic conditions. Also, to study what changes in respiration might occur under such conditions.

## Method

Records of cardiac activity were taken by means of an electrocardiograph, Lead II being chosen as likely to be of most value. In place of the usual technique of compensating for the so-called skin current, use was made of a 20 m.f. condenser in series with the electrodes and the string of the galvanometer, in the first part of the study. Various workers (2, 4) have shown that records taken by this method are comparable to those obtained with the more usual technique of compensation. The condenser method has the advantage of extreme simplicity and ease of operation, and one has not to contend with the shift of the galvanometer string due to skin changes which occur when sudden upset of an emotional nature takes place. Difficulty was experienced owing to the fact that an alternating field of unknown source made itself manifest, and in the course of experimentation it was decided to abandon the condenser method since the alternating effect seemed less marked when the method of compensation was used. Synchronous respiratory records were taken by means of a Sommer pneumograph and a Marey tambour, the lever of which was adjusted so that its shadow was recorded along with the heart record.

The experimental procedure was as follows. The subject was received and seated comfortably in the dark room which contained all of the apparatus; the electrodes were then applied and the pneumograph adjusted. The revolving-drum camera was run at a speed of one meter in 10 seconds for most of the experiments, so that 10 to 15 heart-beats were recorded in each case, time being indicated by a Jacquet time-marker beating fifths of seconds. After the subject was settled, a record of "normal" activity of the heart was taken. The camera was then reloaded and an electrocardiogram secured during the period of upset. Stimulation consisted of the loud noise produced by the falling of a weighted hammer against the side of a large tin drum, this apparatus being placed near the subject's chair but concealed by a screen. A camera contact simultaneously released the magnet holding the hammer aloft and closed a photographically recording signal-marker. Thus, with the stimulus switch closed and the camera drum in motion, the hammer fell two seconds after the camera shutter had opened, the electrocardiogram being recorded

during a revolution of the drum when the shutter closed automatically. This procedure was gone through twice so that four records, two normal and two during stimulation, were obtained from each subject; the same routine of experimentation was followed in each case.

The subjects in this experiment consisted of 8 adults and 2 boys in normal health and 27 psychopathic patients from the wards of Maudsley Hospital, London, all being free from signs of heart disorder. The following is a tabulation of the clinical diagnosis in the phychopathic cases: anxiety neurosis, six cases; dementia praecox, seven; post-encephalitic, six; melancholia, five; hysteria, one; exophthalmic goiter, one; paranoid state, one.

## Results

Seventy-four electrocardiograms were obtained during which sudden, unexpected, auditory stimulation had occurred; also a like number without stimulation as controls. The control records were normal in all subjects. Respiratory curves were taken in all except 36 records.

These electrocardiograms have been analyzed with regard to the following points: (1) difference in form subsequent to stimulation, and a comparison of any such changes in the normal and the psychopathic subjects; (2) the relation between any changes—as in (1)—and the point in the cardiac cycle at which stimulation occurred; (3) the time relationship between systole and diastole (i.e., as measured by the P-T and T-P intervals), and variations in the systolic-diastolic ratio in so far as P-T/T-P gives a measure of such; (4) the time elapsing between stimulation and any resulting variation in the electrocardiogram; (5) changes in respiration.

1) *Variations in the form of the electrocardiogram subsequent to stimulation.* In 20 of the 74 records there were resultant irregularities in the form of the electrocardiogram.

| | | | | |
|---|---|---|---|---|
| Class I | Inverted waves | T (R depressed) | T wave | P wave |
| | Records no. | 2, 7 | 79, 88 | 138 |
| Class II | Small irregular variations, presumably extra-cardiac, during | Systole | Diastole | |
| | Records no. | 8, 71, 94, 100, 142, 144 | 4, 42 | |
| Class III | Heightened waves | S and T | R and T | U |
| | Records no. | 26, 140 | 86 | 114, 136 |

| | | | |
|---|---|---|---|
| Class IV | Extra waves | U | Q R S (ventricular extra-systole) |
| | Records no. | 63 | 80 |

The following is a list of the clinical diagnosis in these cases: normal subjects, 7, 79, 88, 8, 71, 94, 100, 86, 114, 80; post-encephalitic, 138, 142, 144, 140, 136; dementia praecox, 2, 4; anxiety neurosis, 42, 26; hysteria, 63.

The irregularities are interesting in themselves, but no correspondence has been found between any one type and other concomitant factors of which we have a record, namely, period in the cardiac cycle at which stimulation occurred, respiratory cycle, or change in systolic-diastolic ratio. It will be noted that some variation occurred in each of the normal subjects and in all the post-encephalitic cases except one. Also that in only two of the six anxiety cases and in two of the seven cases of dementia praecox did any change occur, and in none of the five cases of melancholia.

2) *The relation between the response and the point in the cardiac cycle at which stimulation occurred.* It was found that, if stimulation occurred during the Q R S complex, subsequent irregularities were attendant in all seven of such records secured; three of the irregularities were of the form of Class I, two of Class II, and one each of Classes III and IV. In four of the ten instances in which stimulation occurred near the mid-point of the T-P interval there were subsequent changes; two of these were of the form of Class III, and one each of Classes I and II. At no other points in the cardiac cycle than these mentioned was there any constant tendency towards resulting alteration, the nine other instances being scattered at random points in the cycle.

3) *Time relations of systole and diastole (as measured by the P-T and T-P intervals).* An examination of the records was made to determine whether the change in absolute time of the P-T or T-P interval was the greater following stimulation. In 39 instances we found that T-P was diminished more than P-T; in 12 cases, the change was more marked—although only slightly so—in P-T; and in 23 cases there was not enough difference to make a distinction. With respect to the point of stimulation, it was found that of the 74 records the stimulus occurred during the T-P interval in 24 cases, and during the P-T interval in 50 cases. In general this analysis confirms the statements of other workers that, following stimulation

which results in an accelerated heart rate, shortening will take place in the diastolic phase whereas the systolic phase is less likely to be affected. It should be stated that there was no apparent relation between the above differences and the clinical state of the subjects.

An attempt was made to find what change in the P-T/T-P ratios occurred following stimulation as compared with the control records. The results are not given in detail but it may be mentioned that two-thirds of the normal records and of the anxiety states did show a change in this ratio, whereas only one-third of the records in the case of dementia praecox, post-encephalitic states, and melancholia showed any change.

4) *The time elapsing between the stimulation and subsequent variation in the electrocardiogram.* The time elapsing between stimulation and the first appearance of any alteration in the form of the electrocardiogram was measured, and the results show the following variations: one case in 0.3 secs., eleven in 0.4 secs., three in 0.5 secs., three in 0.6 secs., two in 0.8 secs., thus giving an average of 0.48 secs. There was no apparent relationship between the elapsed time and the clinical state, or other noteworthy conditions.

5) *Change in respiration.* The respiratory records showed no concomitant relationship to any other factor studied here. Blatz (1) found that the sudden, unexpected stimulus of falling backward in a chair had the effect of always interrupting an expiratory phase in the respiratory cycle and changing it to an inspiratory phase, while stimulation during inspiration did not effect the respiratory cycle. Skaggs (8) found a similar respiratory change to that described by Blatz; Skaggs used the sudden sounding of an automobile Klaxon as a stimulation agent. Our sudden, unexpected stimulus (a loud bang) failed to give this result. We did find occasional irregularities in the respiratory cycle but these occurred as often during expiration as inspiration.

## Summary

This experiment was designed to study the relationship between the emotional experience of surprise and the activity of the heart as shown in the electrocardiogram. The following points have been brought out:

1) "Surprise" stimulation causes changes in form in the electrocardiogram of a variety of types. In all ten normal subjects some variation resulted, but in none of five cases of melancholia and in only two of seven cases of dementia praecox.

2) When the stimulation occurred near the mid-point of the P-T or the T-P intervals of the pulse wave, there was a tendency towards subsequent change in the electrocardiogram.

3) In 39 records the T-P interval showed a greater change than the P-T, subsequent to stimulation; in 12 records the change was more marked in the P-T, although only slightly so; in 23 records there was not enough difference to make a distinction.

4) After stimulation, an alteration in the P-T/T-P ratio was manifest in two-thirds of the records fo normals and anxiety states, but in only one-third of those of melancholic, post-encephalitic, and dementia praecox subjects.

5) The time interval between stimulation and subsequent change in the form of the electrocardiogram varied between 0.3 secs. and 0.8 secs., with an average of 0.48 secs.

6) Records of respiration did not show the alteration recorded by other observers as a result of sudden stimulation. The respiratory changes noted showed no relationship to other factors studied.

## References


1. Blatz, W. E. The cardiac, respiratory, and electrical phenomena involved in the emotion of fear. *J. Exper. Psychol.*, 1925, **8**, 109-132.
2. Dodge, R., & Benedict, F. G. The psychological effects of alcohol. Washington, D. C.: Carnegie Institution of Washington, 1915. Pp. 281.
3. Hunt, R. Direct and reflex acceleration of the mammalian heart, with some observations on the relations of the inhibitory and accelerator nerves. *Amer. J. Physiol.*, 1899, **2**, 395-470.
4. Kahn, R. H. Das Elektrokardiogramm. *Erg. d. Physiol.*, 1914, **14**, 1-252.
5. Landis, C. Studies of emotional reactions: II. General behavior and facial expression. *J. Comp. Psychol.*, 1924, **4**, 447-511.
6. Landis, C., & Gullette, R. Studies of emotional reactions: III. Systolic blood pressure and inspiration-expiration ratios. *J. Comp. Psychol.*, 1925, **5**, 221-224.
7. Robbins, S. D. A plethysmographic study of shock and stammering. *Amer. J. Physiol.*, 1919, **48**, 285-330.
8. Skaggs, E. B. Changes in pulse, breathing and steadiness under conditions of startledness and excited expectancy. *J. Comp. Psychol.*, 1926, **6**, 303-318.


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## ÉTUDE DE RÉACTIONS ÉMOTIVES: VI. RÉPONSES CARDIAQUES

(Résumé)

On a formulé cette expérience pour étudier le rapport entre l'expérience émotive de surprise et l'activité du coeur montrée dans l'électrocardiogramme. On a trouvé les données suivantes:

1) La stimulation par "surprise" cause des changements de forme dans l'électrocardiogramme d'une variété de types. Chez tous les 10 sujets normaux quelque variation a résulté, mais il n'y a eu nulle variation dans 5 cas de mélancholie et une variation dans seulement 2 sur 7 cas de démence précoce.

2) Quand la stimulation a eu lieu près du milieu des intervalles P-T ou T-P de l'onde d'impulsion, il y a eu une tendance vers un changement subséquent dans l'electrocardiogramme.

3) Dans 39 notations l'intervalle T-P a montré un plus grand changement que le P-T, subséquent à la stimulation; dans 12 notations le changement a été plus frappant dans le P-T, bien que seulement légèrement; dans 23 notations il n'y a pas eu assez de différence pour faire une distinction.

4) Après la stimulation, un changement dans la ration PT/TP a été manifesté dans les deux tiers des notations des états normaux et ceux d'anxiété, mais seulement dans un tiers de ceux des sujets mélancholiques, post-encéphalitiques, et ceux souffrant de démence précoce.

5) L'intervalle de temps entre la stimulation et le changement subséquent dans la forme de l'electrocardiogramme a varié entre 0, 3 secondes et 0, 8 secondes, avec une moyenne de 0, 48 secondes.

Les notations de respiration n'ont pas montré le changement noté par autres observateurs comme résultat d'une stimulation soudaine. Les changements respiratoires notés n'ont pas montré un rapport avec les autres facteurs étudiés.

Landis et Slight

## UNTERSUCHUNGEN DER GEFÜHLSREAKTIONEN: VI. KARDIAL REAKTIONEN

(Referat)

Der Zweck dieses Experiments war das Studium der Beziehung zwischen der Gefühlserregung durch Überraschung und der Herztätigkeit, wie das Elektrokardiogramm sie zeigt. Man erhielt die folgenden Ergebnisse:

1) Reizung durch "Überraschung" verursacht Formveranderungen sehr verschiedener Art im Elektrokardiogramm. In allen 10 normalen Vpn. erfolgten gewisse Variationen, aber keine in 5 Fällen von Melancholie, und in nur 2 Fällen aus 7 von Dementia Praecox.

2) Wenn die Reizung nahe dem Mittelpunkt des B-T (Berg-Tal) Intervale oder des T-B Intervals der Pulswelle stattfand, so hatte dies oft Veränderungen im Elektrokardiogramm zur Folge.

3) In 39 Tabellen zeigte der B-F Interval grössere Veränderungen als der B-T Interval, in Folge der Reizung; in 12 Tabellen waren die Veränderungen bedeutender in B-T, doch nur in geringem Masze; in 23 Tabellen waren die Veränderungen zu gering um sie unterscheiden zu können.

4) Nach der Reizung zeigte sich eine Veränderung in der B-T/T-B Rate in zwei Drittel der Tabellen von Normal- und Angst-zuständen, doch nur in einem Drittel der Tabellen in Fällen von Melancholie, Postencephalitis, und Dementia Praecox.

5) Der Zeitinterval zwischen Reizung und der folgenden Veränderung in der Form des Elektrokardiogramm schwankte zwischen 0.3 Sekunden und 0.8 Sekunden, mit einem Durchschnitt von 0.48 Sekunden.

6) Tabellen des Atmens zeigten keine Veränderungen wie andere Beobachter sie als Resultat plötzlicher Reizung verzeichnet haben. Die Veränderungen im Atmen erwiesen keine Beziehungen zu andern Faktoren die man untersuchte.

LANDIS UND SLIGHT

## ИЗУЧЕНИЕ ЭМОЦИАЛЬНЫХ РЕАКЦИЙ. VI. РЕАКЦИЯ СЕРДЦА.

(Реферат).

Этот эксперимент был предпринят в целях изучения взаимоотношений, существующих между эмоцией удивления и деятельностью сердца, записанной на электрокардиограмме. Были установлены следующие положения:

1) стимул, вызывающий „удивление", вызывает различные изменения в форме электрокардиограммы. Изменения наблюдались во всех 10 нормальных случаях, но в 7 случаях шизофрении было наблюдено только 2 изменения, а в 5 случаях меланхолии—ни одного;

2) когда раздражение имело место около средней точки интервалов P — T или T — P пульсовой волны, то наблюдалась тенденция к немедленным изменениям электрокардиограммы;

3) в 39 случаях интервал T — P показал большие изменения, чем интервал P — T; в 12 случаях легкое преобладание было на стороне интервала P — T, в 23 случаях разницы почти не было;

4) изменения отношения PT/TP после раздражения составляли 2/3 в случае нормальных и возбужденных испытуемых и только 1/3 в случае меланхолии, пост-энцефалитических состояний и шизофрении;

5) промежуток времени между раздражением и изменением формы электрокардиограммы варировал между 0,3 сек. и 0,8 сек. при среднем в 0,48 сек.;

6) наблюдения дыхания не показали изменений, отмечавшихся другими исследователями после внезапных раздражений. Отмеченные дыхательные изменения стояли вне всякой связи с другими изучаемыми факторами.

**Лэндис и Слайт (Landis and Slight).**

# A QUANTITATIVE STUDY OF THE SIMILARITY FACTOR IN RETROACTIVE INHIBITION*

*From the Psychological Laboratories of Clark University*

LUBERTA M. HARDEN

## INTRODUCTION

Upon the basis of all the experimental work that has been done in the field of retroactive inhibition only two conclusions seem justified. In the first place, an activity interpolated between learning and recall does interfere with that recall. In the second place, the degree of such interference varies with certain relationships between the original and the interpolated activities. Just what the important factors in these relationships are has not been determined, although a number of experimental studies have been made in the effort to discover the basic principles.

One factor which has seemed important in this regard is the similarity between the original and the interpolated activities. Rather definite laws or principles have been suggested by Robinson (1) and Skaggs (3) and at least partly substantiated by Whitely (4). Recently, however, Robinson (2) has made a new experimental attack on the problem. In this study he fails to verify the theoretical relationship between similarity and retroaction which he and Skaggs had previously postulated.

The theoretical relationship, Robinson (2, p. 298-299) says, seems to be something like this: "As similarity between interpolation and original memorization is reduced from near identity, retention falls away to a minimum and then rises again, but with decreasing similarity it never reaches the level obtaining with maximum similarity." This relationship is graphically represented in Figure 1 (copied from Robinson).

Robinson (2, p. 300) has shown experimentally that "where the interpolated activities can be ranked according to their similarity to the original learning, the recall values fell in the inverse rank order. That is to say, the greater the similarity, the less efficient

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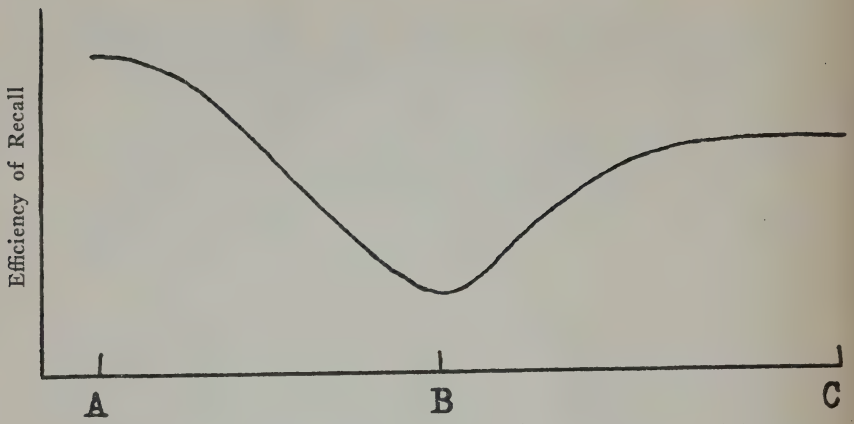


FIGURE 1
SIMILARITY AND EFFICIENCY OF RECALL
(From Robinson)

"At A, where similarity between interpolated activity and original mmorization is at a maximum (where they are as nearly as possible identical), recall is at its highest level of efficiency. As this similarity approaches its minimum at C it passes through an intermediate degree, B, where recall is exceedingly inefficient. After this point is passed, there is an increase in efficiency of recall, but this increase does not return to the level obtained at A" (2, p. 299).

the recall." He quotes the following as an example: ". . . memorization of a set of 8 four-place numbers was followed by memorization of another set of such numbers or by memorization of a column of 32 single digits or by memorization of poetry. The rank order according to similarity is here perfectly definite and so were the recalls attained after these different types of interpolation." He points out two faults in such an experimental test of the similarity problem: (1) In the first place it shows but one phase of the presumed relationship, the increase of recall with increased dissimilarity (*B* to *C* on the curve in Figure 1); it does not show the variation from near identity to non-identity of the same type of material (*A* to *B* on the curve in Figure 1). (2) The second difficulty is that the similartiy of the activities employed, i.e., learning four-place numbers, memorization of digits, or of poetry, etc., can never be measured. These processes can be ranked only on a similarity-dissimilarity scale. Hence results obtained from experiments utilizing such materials can be quantitative only in a very rough sort of way.

In order to avoid these difficulties Robinson devised an experimental

situation in which he used but one type of material (consonants) and varied the degree of identity between the original and interpolated lists in a manner which may be *schematically* shown as follows:

| Degree of Identity | Arrangement of Items | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| None in common | a | b | c | d | e | f | g | h |
| One in common | *a* | b | c | d | *a* | f | g | h |
| | a | *b* | c | d | e | *b* | g | h |
| | a | b | *c* | d | e | f | *c* | h |
| | a | b | c | *d* | e | f | g | *d* |
| Two in common | *a* | *b* | c | d | *a* | *b* | g | h |
| | *a* | b | *c* | d | *a* | *f* | *c* | h |
| | *a* | b | c | *d* | *a* | f | g | *d* |
| | a | *b* | *c* | d | e | *b* | *c* | h |
| | a | *b* | c | *d* | e | *b* | g | *d* |
| | a | b | *c* | *d* | e | f | *c* | *d* |
| Three in common | *a* | *b* | *c* | d | *a* | *b* | *c* | h |
| | *a* | b | *c* | *d* | *a* | f | *c* | *d* |
| | a | *b* | *c* | *d* | e | *b* | *c* | *d* |
| | *a* | *b* | c | *d* | *a* | *b* | g | *d* |
| Four in common | *a* | *b* | *c* | *d* | *a* | *b* | *c* | *d* |

It should be remembered that the actual experiment utilized only consonants. The first four members of the series of eight consonants represented the original series, the second group of four were considered as the interpolated list. The experimental method employed will not be described at this point, for that described later in the experimental section of this paper duplicated it as nearly as possible.

The results obtained by Robinson with this arrangement of material do not show the theoretical relationship of Figure 1. The curves of percentage of recall do not characteristically display the final rise shown in that figure (*B* to *C*). He therefore concludes that (2, p. 312) "however many may be the conditions under which there is a first order inversion in the debated function, there are demonstrable conditions under which such inversion is absent."

The failure of Robinson's results to display the final increase in retention seems to the writer only to be expected, for it appears that he has carried the experiment only through the first phase, i.e., from *A* to *B* in Figure 1, the phase that has been neglected heretofore. In former studies both he and Skaggs have used the term similarity to indicate the same type of material and dissimilarity to indicate a different type of material. For instance, if nonsense syllables were used as original learning material and a second list of nonsense syllables were

used as interpolated material, the materials were described as similar. On the other hand, if three-place numbers, as interpolated material, followed nonsense syllables, as original material, the relationship was described as dissimilar. It is clear that according to this concept of similarity and dissimilarity, Robinson dealt only with similar materials in his recent study, since he used consonants as both original and interpolated materials.

The purpose of the present paper is to supplement Robinson's study by introducing varying degrees of dissimilar material (digits) in the interpolated lists, using consonants as original learning material. Robinson's results are accepted as showing the proper relationships for the various degrees of identity. The present paper will show the effect of varying the digit-consonant make-up of the interpolated list in the same manner that Robinson varied the identical–non-identical consonant make-up of his interpolated lists. Speaking in terms of the curve in Figure 1, the purpose of this paper is to determine experimentally the course of the curve approximately from *B* to *C*.

The question will undoubtedly be raised as to whether or not consonants and digits are really dissimilar. The answer can be given only in terms of the results of this experiment. Four digits presented immediately after four consonants affect the recall of those consonants less than do four other consonants presented under the same circumstances as were the digits. In this respect, then, we may say that digits and consonants are dissimilar.

## The Experiment

*The Problem.* Specifically stated, the problem studied was this: How does the interference in the recall of four consonants vary with the consonant-digit composition of a list of four members presented immediately following the presentation of those consonants?

*Materials.* Two series of 30 lists each were prepared. Each list was composed of eight members, the first four of which were consonants selected at random except that particular pains were taken to avoid certain relationships such as familiar combinations, rhymes, well-known abbreviations, initials, etc. These four consonants corresponded to the original learning material of the typical experimental study of retroactive inhibition. The remaining four members of each list served as the interpolated material and consisted of various arrangements of consonants and digits. The composition of the lists may be *schematically* shown as follows. It must be remembered that the letters represent random arrangements of consonants, and that the digits were presented in random order, no

two occurring in regular ascending or descending order, and such arrangements as 2, 4, 6, or 3, 5, 7, or 3, 6, 9, etc., being carefully avoided.

| Degree of Similarity | Arrangement of Items | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| None in common | a | b | c | d | 1 | 2 | 3 | 4 |
| One in common | a | b | c | d | e | 2 | 3 | 4 |
| | a | b | c | d | 1 | f | 3 | 4 |
| | a | b | c | d | 1 | 2 | g | 4 |
| | a | b | c | d | 1 | 2 | 3 | h |
| Two in common | a | b | c | d | e | f | 3 | 4 |
| | a | b | c | d | e | 2 | g | 4 |
| | a | b | c | d | e | 2 | 3 | h |
| | a | b | c | d | 1 | f | g | 4 |
| | a | b | c | d | 1 | f | 3 | h |
| | a | b | c | d | 1 | 2 | g | h |
| Three in common | a | b | c | d | e | f | g | 4 |
| | a | b | c | d | e | 2 | g | h |
| | a | b | c | d | 1 | f | g | h |
| | a | b | c | d | e | f | 3 | h |
| Four in common | a | b | c | d | e | f | g | h |

It will be seen that there were thus all degrees of similarity as to content. Such an arrangement calls for only 16 lists. However, the fact that the conditions are unequally represented in this program had to be taken into account in order to make results for all conditions comparable. Since the maximum number of lists required in any one condition to take care of the position factor was six, this was used as a basis, and six lists were prepared for each of the five conditions. This accounts for the 30 lists which were mentioned above as comprising each of the two series.

*Method.* The experimental procedure followed that described by Robinson as closely as possible. The memory-span method was used, each list being exposed but once. Presentation was made visually by means of a Spindler-Hoyer exposure apparatus. The subject was instructed to pronounce aloud each consonant or digit as it appeared. Each member of the list was exposed for .5 second, and there was a .5-second interval between the members. The total exposure time for each list was therefore about 8 seconds. An entire experimental sitting in which the 30 lists were used required approximately 30 minutes. It is not likely that the factor of fatigue was important. However, in order to equalize any possible effects of that factor or of practice, the lists were presented in an irregular

order with reference to the degrees of similarity. Written recall for each list followed immediately after the exposure of the eighth member of the list.

Each subject served four times and was given the complete series of 30 lists at each sitting. At the beginning of the first sitting a trial series of five lists was given to each subject to acquaint him with the method. Since the memory-span method was used and since it seemed very unlikely that learning occurred, it seemed permissible to use each series twice, so Series I was given on the first and third experimental days and Series II on the second and fourth days.

Scoring was based on the correctness in kind and position of each of the first four consonants of each list.

*Subjects.* Eight graduate students in the Department of Psychology, one Professor of Psychology, and one secretary in the Department, a college graduate, served as subjects. All subjects knew the general nature of the experiment, but none knew the details. Most important of all, no one knew that only the first four members of the list were to be scored.

*Results.* The results in terms of percentage of recall of consonants correct in kind and position are presented in Table 1. The figures for the individual subjects represent the average percentage of correct recall of 96 consonants under the condition specified (four consonants in each of the six lists which were presented at each of the four experimental sittings). Since there were 10 subjects, $N$ for the general averages under each condition is 960.

TABLE 1

PERCENTAGE OF CORRECT RECALL FOR EACH SUBJECT UNDER EACH CONDITION AND AVERAGES FOR THE GROUP

| Degree of similarity | 4 | 3 | 2 | 1 | 0 |
|---|---|---|---|---|---|
| 1 | 76 | 68 | 77 | 84 | 94 |
| 2 | 51 | 49 | 31 | 33 | 58 |
| 3 | 59 | 62 | 57 | 63 | 80 |
| 4 | 80 | 74 | 82 | 71 | 90 |
| 5 | 68 | 51 | 69 | 61 | 83 |
| 6 | 56 | 61 | 59 | 55 | 85 |
| 7 | 80 | 87 | 77 | 82 | 92 |
| 8 | 100 | 100 | 95 | 100 | 98 |
| 9 | 52 | 48 | 51 | 41 | 79 |
| 10 | 70 | 66 | 77 | 64 | 81 |
| Averages | 69.2±1.01 | 66.6±1.02 | 67.5±1.01 | 65.4±1.03 | 84.0±0.08 |

From these data it appears that recall is considerably better under the none-in-common condition than under any other. The group averages show differences between this condition and others varying from 14.8% to 18.6% with a P.E.$_{diff.}$ of 1.3% in each case. Of the individual records all except that of Subject 8 show the same tendency. It may be stated here that the record of this subject shows that the experimental situation was too limited in extent to furnish a test of his memory span, only three errors occurring during the entire series. His record is therefore not significant for our problem.

A casual observation would indicate that the four-in-common condition, that is, the condition of greatest similarity between original and interpolated lists, does not produce the greatest degree of interference, but that the mixed material is more effective in this respect. However, the reliability data show that the differences between the one-, two-, three-, and four-in-common conditions are really not significant, the differences varying from 0.9% between the two- and three-in-common conditions to 3.8% between the one- and four-in-common conditions with a P.E.$_{diff.}$ of 1.6% in each case. The individual records show great variability on this point.

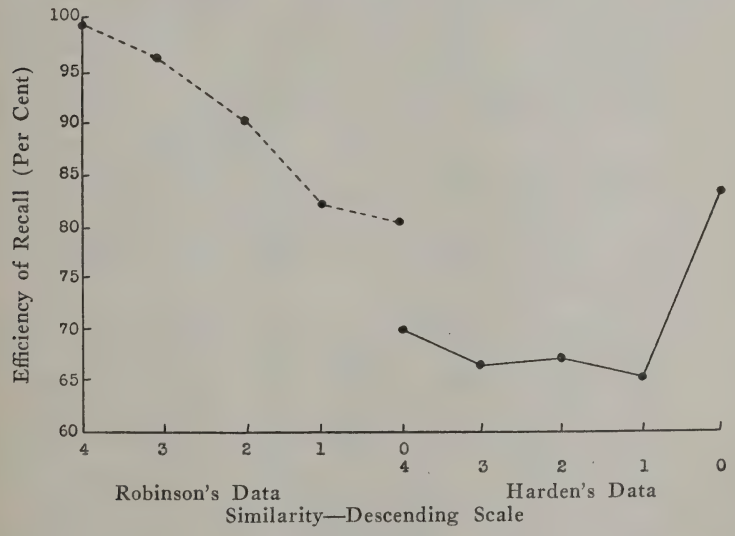


FIGURE 2

DEGREE OF SIMILARITY AND EFFICIENCY OF RECALL AS SHOWN BY ROBINSON'S RESULTS AND THOSE OF THE PRESENT STUDY

The general tendency as shown by the data obtained is clearly seen from the graphic representation in Figure 2. In order to show the relationship between Robinson's results and those of the present study, a copy of his curve is shown in dotted lines on the same graph. As explained above, and as indicated on this graph, the none-in-common condition of Robinson's study corresponds to the four-in-common condition of this study. The discrepancy in the percentage of recall for this condition in the two studies (approximately 80% for Robinson and 69.2% in this study) is probably due to unavoidable differences in the experimental conditions. Robinson's data were obtained from an experimental class of 20 students, who worked in couples, serving alternately as *E* and *O*, while the writer acted as *E* all through the present experiment. The exposure apparatuses were probably different also. The averages for the none- and the four-in-common conditions of this study are based upon more data than Robinson had for these conditions. However, for the one-, two-, and three-in-common conditions he had more data than was used in the present averages.

It will be seen that the course of that part of the curve which corresponds to *B* to *C* on Figure 1 follows the theoretical curve in one respect only, it does display a final rise. The shape of this part of the curve, however, is quite different from that of the theoretical curve. It shows a very sharp final rise from a rather persistent low level, rather than the relatively rapid initial rise from *B*, and gradual leveling off as the curve approaches *C*, the point of greatest dissimilarity. In other words, the curve obtained from these data is concave, while the theoretical curve is convex from *B* to *C*. However, these data should be regarded as suggestive and not final. Further experimentation is necessary to decide the true course of this part of the curve. It would be desirable to use more degrees of similarity in such work.

*The Possibility of the Significance of the Factor of Difficulty in these Experimental Results.* Voluntary reports of the subjects who served in this experiment have led the writer to think that perhaps the factor of difficulty was important in this work. Without exception they stated that the none-in-common lists were easier to remember than the others. Complete records were not kept of the recall of the entire eight members of the lists. However, using the records from six sittings, i.e., the recall of 288 consonants and digits (eight in each of six lists under each condition in each of the six

records), the following percentages of correct recall for the eight members are obtained:

| | |
|---|---|
| None in common | 88.9% |
| One in common | 67.7% |
| Two in common | 62.2% |
| Three in common | 59.4% |
| Four in common | 59.4% |

Do these figures mean that digits are more easily remembered than consonants? If so, the effective factor may have been the relative difficulty of the interpolated material rather than the degree of similarity between original and interpolated materials. This factor might also have been operating in Robinson's experiment, since it would undoubtedly be easier to remember the four-in-common lists, and others in descending order, for the first four consonants were either wholly or in part repeated in the last four members of the lists.

In the present experimental situation the relative importance of difficulty and similarity can be determined by reversing the position of the consonants and digits in the lists. Then if the digits are simply more easily remembered than consonants, we might expect to obtain results showing the most efficient recall on the four-in-common (eight digits, the "similar" condition). If, on the other hand, similarity is the important factor, the most efficient recall should occur with the none-in-common condition, as in the present experiment. Such an experiment has been planned by the writer, but as yet it has not been carried out.

*Summary and Conclusions.* In this experiment an attempt has been made to study the similarity factor quantitatively by the memory-span method. Two types of material, consonants and digits, and five degrees of similarity between original and interpolated lists were used.

The results obtained show that recall is more efficient when the original and interpolated materials are most dissimilar than when these materials are similar or mixed. When combined with the results of Robinson's recent experiment, they indicate that the theoretical relationship represented in the curve in Figure 1 holds in part. Robinson's results show that the relationship shown from *A* to *B* on the curve (from identity to non-identity of original and interpolated materials) is probably the true one. The results of the present study indicate that there is a final rise in the curve as shown

from *B* to *C* on the theoretical curve, but they suggest that the shape of the curve between these points may be concave rather than convex.

It has been suggested by remarks of subjects and by some experimental data that the relative difficulty of the interpolated material may have been a factor in determining the degree of inhibition shown in the results of this study. Further work has been planned to determine whether or not this is the case.

## References

1. Robinson, E. S. Some factors determining the degree of retroactive inhibition. *Psychol. Monog.*, 1920, **28**, No. 128. Pp. 57.
2. ————. The 'similarity factor' in retroaction. *Amer. J. Psychol.*, 1927, **39**, 297-312.
3. Skaggs, E. B. Further studies in retroaction inhibition. *Psychol. Monog.*, 1925, **34**, No. 161. Pp. 60.
4. Whitely, P. L. The dependence of learning and recall upon prior intellectual activities. *J. Exper. Psychol.*, 1927, **10**, 489-508.

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## UNE ÉTUDE QUANTITATIVE DU FACTEUR SIMILARITÉ DANS L'INHIBITION RÉTROACTIVE

(Résumé)

Les études antérieures de l'inhibition rétroactive ont montré que des activités interpolées entre l'apprentissage et le rappel empêchent ce rappel. Le degré de cet empêchement varie avec nombre de facteurs, parmi lesquels le degré de similarité entre la première matière à apprendre et la matière interpolée semble important. Cette étude a considéré le facteur similarité quantitativement par la méthode de durée de mémoire comme employée par Robinson dans son étude récente de ce facteur (*Amer. J. Psychol.*, 1927, **39**, 297-312). On a employé deux sortes de matières (consonnes et chiffres) et 5 degrés de similarité. On a présenté visuellement une liste de 8 membres. Les quatre premiers membres ont été toujours consonnes et ont correspondu à la prémière matière à apprendre. Les quatre seconds membres ont été considérés comme matière interpolée et ont été composés de groupes de consonnes et de chiffres variant de "0- à 4-en-commun" avec la première matière, c'est-à-dire, de 0 à 4 consonnes. Il a fallu six listes pour le facteur de position des consonnes parmi les chiffres dans le listes interpolées; on a pris cela comme base, et on a préparé 6 listes pour chaque condition. Il y a eu ainsi 30 listes dans une série. Dix sujets ont étudié toutes les 30 listes à chacune de quatre séances. Les résultats indiquent que le rappel est le plus efficace quand la première matière et celle interpolée ne sont pas semblables, c'est-à-dire, dans la

condition 0-en-commun. Les pourcentages du rappel correct pour les cinq conditions sont les suivants: 0-en-commun, 84+.08; 1-en-commun, 65.4+1.03; 2-en-commun, 67.5+1.01; 3-en-commun, 66.6+1.02; et 4-en-commun 69.2+1.01.

HARDEN

## EINE QUANTITATIVE STUDIE DES ÄHNLICHKEITSFAKTORS IN RÜCKWIRKENDER HEMMUNG

(Referat)

Frühere Studien der rückwirkenden Hemmung haben bewiesen dass eine Tätigkeit die zwischen dem Lernen und der Wiederholung eingeschoben wurde, hemmend auf die Erinnerung wirkt. Die Stärke dieser Hemmung variert mit der Anzahl der Faktoren, wobei der Grad der Ähnlichkeit des erlernten Materials mit dem eingeschalteten Material von Wichtigkeit zu sein scheint. In dieser Studie wird der Ähnlichkeitsfaktor quantitativ behandelt und zwar nach der "Gedächtnisspanne" Methode, wie Robinson sie letzthin in seiner Studie dieses Faktors anwandte (*Amer. J. Psychol.*, 1927, **39**, 297-312). Man benutzte zweierlei Material (Konsonanten und Zahlen) und 5 Grade der Ähnlichkeit. Man zeigte den Vpn. Listen von 8 Zeichen wovon die ersten 4 immer Konsonanten waren und dem gewöhnlichen Lernmaterial entsprachen. Die letzten 4 Zeichen stellten das eingeschaltete Material vor und bestanden aus Konsonanten und Zahlen die von "0 bis 4 gemeinsam" variierten von dem Original-material, i.e., von 0 bis 4 Konsonanten enthielten. Sechs Listen waren nötig den Faktor der Stellung der Konsonanten unter den Zahlen der eingeschalteten Listen zu bestimmen; dies wurde als Basis angenommen und für jeden Fall machte man sodann eine Liste, folglich 30 Listen für jede Serie. Zehn Vpn. studierten alle 30 Listen bei jeder der vier Sitzungen. Die Ergebnisse deuten an, dass das Gedächtnis am wirksamsten fungirt wenn das erlernte Material und das eingeschaltete Material verschieden sind, i.e., bei dem Fall "0-gemeinsam." Der Prozentsatz der korrekten Wiederholung für die 5 Fälle war wie folgt: 0-gemeinsam, 84±.08; 1-gemeinsam, 65.4±1.03; 2-gemeinsam, 67.5±1.01; 3-gemeinsam, 66.6±1.02; und 4-gemeinsam, 69.2±1.01.

HARDEN

## КОЛИЧЕСТВЕННОЕ ИЗУЧЕНИЕ ФАКТОРА СХОДСТВА ПРИ РЕТРОАКТИВНОМ ТОРМОЖЕНИИ.

(Реферат).

Предыдущие исследования ретроактивного торможения показали, что действия, производимые между заучиванием и воспроизведением, мешают этому воспроизведению. Степень этого торможения зависит от многих факторов, самым важным из которых можно считать степень сходства первоначально выученного материала, с материалом, выученным в промежуток между первоначальным заучиванием и воспроизведением. Данное исследование имело дело с количественным измерением этого фактора по методу, примененному Робинсоном в его последней работе на эту тему (American Journal of Psychology, 1927,39, 297 — 312). Было использовано 2 вида материала (согласные и цифры) и 5 степеней сходства. Для зрительного восприятия был предявлен лист из 8 элементов, первые 4 элемента были согласные и совпадали с принятым в опыте материалом первоначального заучивания. Вторые 4 элемента рассматривались как промежуточный материал и состояли из согласных и чисел, вариирующих в условных единицах от 0 до 4 в отношении с первоначальным материалом. Потребовалось 6 таких серий для того, чтобы учесть фактор положения согласных среди чисел в промежуточных сериях; это было взято за основу при приготовлении 6 серий для каждой комбинации. Получилось 30 последовательных серий. Десять испытуемых запоминали все 30 серий на каждом из четырех сеансов. Результаты показали, что воспроизведение бывают наилучшим, когда первоначальный и промежуточный материл абсолютно несходен, т.-е. при нулевом коэффициенте сходства. Проценты правильных воспроизведений для пяти комбинаций таковы: 0 сходства—84 $\pm$ 08; 1 сходства — 65 . 4 $\pm$ 1.03; 2 сходства — 67.5 $\pm$ 1.01; 3 сходства — 66.6 $\pm$ 1.02 и 4 сходства — 69.2 $\pm$ 1.01.

**Гарден (Harden).**

# NOEGENESIS*

P. B. Ballard

Noegenesis is a word invented by Professor Spearman, and its invention was a necessity. For a new psychological theory had been brought into the world, and the new theory needed a new name; none of the old names would fit. The purpose of this article is to explain what the new theory is and to show what it means for the future of psychology and of the allied sciences of logic and education.

Before Dr. Spearman published his great book (2) in 1923, the psychological doctrine of knowledge (the theory of how each individual mind thinks and knows) was in a discreditable state. The old theories had been abandoned, and no new theories had arrived to take their place. Nobody any longer believed that the association of ideas really explained anything; nobody any longer believed that the doctrine of faculties was anything more than a popular attempt to classify mental processes, and a very clumsy attempt at that—bad as classification and worse still as a principle of explanation. At the same time everybody used the same terms, everybody talked about perception, memory, imagination, and so forth, as though they meant distinct and separate processes with no common elements and no common laws. The popular terms were used because no better terms were available.

Moreover, psychological interest had been diverted into other channels. The theories of Freud, Jung, and Adler seemed to offer a fruitful field of labor to those who were in a hurry to apply psychology to everyday life and saw in psychoanalysis an opportunity to enhance the health and happiness of mankind. Hence the unconscious was studied rather than the conscious, the emotional and volitional side of mind rather than the cognitive side. And applied psychology flourished at the expense of pure psychology. But pure psychology cannot be ignored without serious detriment to the practical issues, nor can the theory of thinking be set aside until problems of feeling and willing have been solved. In fact two interesting

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excursions were made into the neglected realm—one from America and the other from Germany. In America the result was behaviorism, in Germany the Gestalt psychology. The American adventure need not detain us long. In the hands of the behaviorists, psychology committed suicide and nothing was left to study but the physical organism. Psychology abdicated in favor of physiology. The Gestalt theory, however, is quite another matter. Here we have a genuine attempt to place the psychology of thought on a new basis. The old categories have been left behind and a search made for new laws and new principles of explanation. And there is little doubt that Wertheimer, Köhler, Koffka, and others of the school have pushed psychological inquiry along the right road. They have, generally speaking, moved in the right direction, but they have not gone far enough. Like the followers of the older school, they have failed to find ultimate laws, failed to reach ultimate elements.

Meanwhile Dr. Spearman, Professor of the Philosophy of Mind at the University of London, has been attacking the problem in his own way. The main purpose of his investigations was to find out how the human intellect is constituted; and for the last quarter of a century he has organized and directed a large number of researches and experiments, all serving the same central purpose, all bearing on the psychology of knowledge. The subject is not to be confused with the theory of knowledge. It has at present nothing to do with an inquiry into the validity of human knowledge in general. It limits itself to the purely psychological aspect of the question and leaves the metaphysical aspect severely alone.[1] It is specially concerned in answering the question: How did knowledge come into existence in the individual mind? How did you and I come to know what we know? There are many other questions involved as well, but this is the central question. And if one word could give Professor Spearman's answer, that word would be noegenesis (Gr. *νοεῖν* = to think, perceive; + *γένεσις* = origin). Noegenesis is the process by which new knowledge enters the human mind. Old knowledge may reappear through reproduction, but new knowledge can only come by way of noegenesis. It is in his masterly analysis of this process that Dr. Spearman's special contribution to the theory of cognition lies.

[1]It does not follow that the theory of noegenesis has no bearing on epistemology. Indeed its philosophical implications are numerous and profound. To reveal them, however, is a task of the future.

He has attacked the problem from a new angle. Instead of taking the current categories of mind for granted—instead of assuming that intelligence, memory, imagination, reason, and such like are valid as scientific classifications of mental phenomena—he has challenged the whole traditional system. He has tried to examine the mind *de novo* and to find out by a new method its natural lines of cleavage. He starts from the fact end, not the name end. Instead of testing and measuring a man's memory, perception, imagination, or what not, he tries to find what factors in the mind have real functional unity; and his tests and measurements and statistical analyses are but means by which he achieves this end. In other words his quest has been for elements and for simple fundamental laws. When he has found them he discovers that they cannot fit in with the current terminology. The old bottles will not hold the new wine.

The trouble with current psychology is (and this is just as true of the newer Gestalt psychology as of the older faculty psychology) that the analysis is incomplete. It is quite true that we have faculties—that we observe, remember, reason, and so forth, but these processes are neither simple nor independent; they are highly complex, and the elements found in any of them are to be found in others as well. It is quite true, as our Gestalt friends tell us, that what we really cognize is not a mass of separate elements but rather a plan or pattern which binds those elements together into one psychical whole. But the patterns which the mind is capable of wearing are innumerable, and we can make no real progress till we discover the simple laws on which they are constructed. Neither the theory of faculties nor the theory of shapes had arrived at the elemental units. They had perhaps reached the molecules but not the atoms, or, to be quite modern, the atoms but not the electrons. The honor of finding the electrons belongs to Professor Spearman. His electrons are "characters" and "relations."

What precisely is Spearman's doctrine of noegenesis? The simplest way to understand it is to study in turn his three simple noegenetic laws or principles.[2] The first is called the *apprehension of experience;* the second, the *eduction of relations;* and the third, the *eduction of correlates.* The first runs thus: *Any lived experience tends to evoke immediately a knowing of its characters and experiencer,*

[2]These laws are fully expounded in Spearman's two books (2, 1).

or, as Spearman expresses it elsewhere, *of its direct attributes and experiencer.* By a "character" is meant any attribute of the outer or inner world that can become an object of thought. Spearman himself puts the law very clearly and simply in the following words: "A person has more or less power to observe what goes on in his own mind. He not only feels but knows what he feels; he not only strives, but knows that he strives; he not only knows, but knows that he knows." Expressed in this way it seems so obvious as to be axiomatic. And yet hidden beneath the surface are many points of ancient controversy. There lies the distinction between experiences and awareness, the question whether consciousness does or does not necessarily involve self-consciousness, and the more pressing questions of today—the relations between the unconscious and the conscious. Experience means to Spearman a much wider thing than knowledge; it merely *tends* to become knowledge, and it actually *does* become knowledge when it is intense enough to rise above the threshold of consciousness. Knowledge has degrees of clearness. The clearness may sink to zero and the experience remain, though the knowledge has vanished. Room is thus left in the noegenetic theory for those realms which have been variously called the subconscious, the unconscious, and the foreconscious.

The second principle more manifestly breaks new ground. It is formulated thus: *The presenting of any two or more characters tends to evolve immediately a knowing of relation between them.* This law "states that when a person has in mind any two or more ideas (using the word to enhance any items of mental content, whether perceived or thought of) he has more or less power to bring to mind any relations that essentially hold between them." "It is instanced whenever a person becomes aware, say, that beer tastes something like weak quinine, that the number seven is larger than five, or that the proposition 'All A is B' proves the proposition 'Some A is B.' For the attributes involved here—resemblances, comparative size, and logical evidence—are undeniably of the kind called relations" (1, p. 165).

Spearman uses the accompanying diagram (Figure 1) to illustrate this principle. *A* and *B* represent the characters that are presented to the mind and *C* represents the relation that tends to become known between them. *A* and *B* are called the "fundaments"—the basic items between which the relation holds. If the relation between the fundaments is grasped by the mind, "eduction" has taken

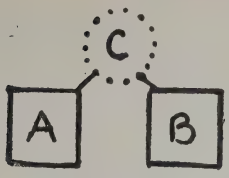


FIGURE 1

place. Using the word "see" in its figurative as well as in its literal sense, we may say that there are three possibilities: A person may see *A* and *B* without seeing any relation between them, that is, without seeing *C*; or he may see *A* and *B* first, and then after a while see *C*; or he may see *A*, *B*, and *C* all at the same time, or so nearly at the same time that it is impossible to divide the processes into two phases. Thus eduction *tends* to take place. It may not take place at all, it may take place after a pause, or it may take place immediately.

We now come to the third and last principle—the eduction of correlates: *The presenting of any characters together with a relation tends to evoke immediately a knowing of the correlative character.* In other words, "when a person has in mind any idea together with a relation, he has more or less power to bring up into mind the correlative idea. For example, let anyone hear a musical note and try to imagine the note a fifth higher. If he understands the relation of "fifth" and moreover possesses an ear for music, he will more or less accurately accomplish the task" (1, p. 166). With the same symbolism as before the educing of correlates is illustrated in Figure 2.

As in the former diagram, the continuous line represents what is originally given, and the dotted line what is educed. It is in the discovery of this last principle and in the realization of its significance and import that Spearman has most markedly shown his originality.

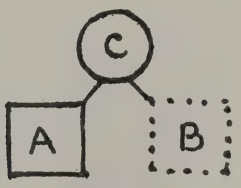


FIGURE 2

The question of relations in general was by no means ignored by the earlier psychologists. Though they did not see the full meaning of this special kind of cognitive process, they did not leave it wholly out of account. The question of correlates, however, which is equally important and equally pervasive of the mental life of man, seems almost entirely to have escaped their notice. And indeed the whole theory of relationship, even by those who dealt with it most adequately, was regarded as quite subsidiary to certain other principles of mental process. It is true that the Gestalt school of psychologists, to which in a sense Spearman himself belongs, has recently raised form or pattern to its proper level or importance, but it has neglected to analyze form into the simple relations of which it is composed. It has been left for Spearman to do this and to formulate in the simplest way the laws that underlie the development of knowledge in the mind of man.

These three noegenetic laws may fitly be compared to Newton's three laws of motion. Just as Newton's laws of motion cover the whole field of physics, so do Spearman's laws of noegenesis cover the whole field of cognition, except in so far as cognition is nearly reproductive. If we leave memory out of account and consider solely the acquisition of entirely new knowledge, the three processes corresponding to the three laws make up all the cognition of which the human mind is capable. And just as at first sight Newton's laws seem very simple and obvious, but their bearing upon the behaviorisms of physical objects is seen only after close and detailed study, so do Spearman's principles readily gain one's assent on first acquaintance, but need to be specially studied in relation to the numerous problems of pure and applied psychology if we are to see how they illuminate the dark places and make clear what was before vague and perplexing.

It must not be thought that the material on which the two eductive processes work is as simple as the above exposition would lead us to believe, for the product of one eduction may become a fundament of another. If *A, B, C,* and *D* in Figure 3 are the fundaments from which we start (we must start somewhere), the relations between them may become the basis of new eductions. The figure indicates but a few of the possible relationships, and merely suggests their possible complexity. We may indeed pile up an indefinite number of relations and correlates and form a huge fabric of constructive thought, which terminates in motions widely removed from the ele-

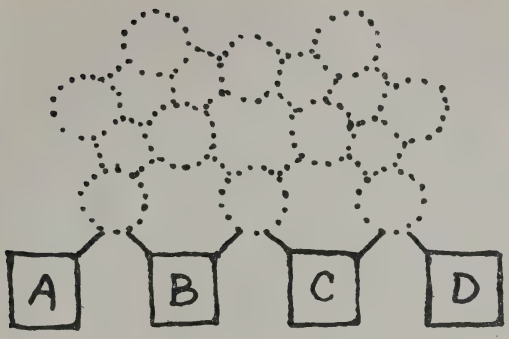


Figure 3

mentary notions from which we started. Nor must it be thought that the construction of this fabric applies to reasoning only. It operates over the whole field of cognition from the following of a cinema film to the understanding of a picture by Velasquez, a symphony by Beethoven, or a play by Shakespeare.

Leaving the realm of pure psychology, let us consider the implication of Spearman's theories for logic and education. Logic is supposed to analyze the process of reasoning and to supply norms by which its validity may be established. But what is "reasoning"? Can animals reason? The mere fact that we still argue over this question indicates the general haziness of our opinions on the matter. It isn't that we don't know what animals can do, but that we don't know whether to call it reasoning. The one thing upon which all are agreed is that in reasoning we somehow or other derive a new idea from old ideas—and this without further recourse to experience. Whether consciously or unconsciously, whether spontaneously or by deliberate effort, we "draw out" from something which is given something which is not explicitly but only implicitly given. The traditional term for this process is "inference" or "deduction." Spearman has employed the more appropriate term "eduction" and has defined it more closely. Eduction is shown to be the primary process that underlies all forms of reasoning. It is the only process that is common to all. It was at one time believed that the Aristotelian syllogism was the basic type to which all true reasoning conformed. It was later realized that deductive logic was only part of the process by which we arrived at truth. The longer process was called

induction or the method of scientific inquiry—a process that begins earlier than deduction and includes it. It begins not with the statement that all A is B but with the evidences on which that statement is based. It begins, in fact, with particulars and not with generalities. The pertinent fact, however, is that neither inductive logic nor deductive logic could be made to cover the whole field. There were innumerable instances which manifestly come under the head of reasoning, and yet could neither be pressed into the syllogistic mold nor explained by the newer principles of induction. Even pure deductive reasoning such as is common in mathematics often remained outside the pale of the recognized laws. When a type of reasoning is so common as to be readily identified, it received a label, such as the *a fortiori* argument for instance. The *a fortiori* argument, however, is but one type among a multitude of nameless types which have resisted all attempts at classification. And the skeleton key to the whole multitude has been supplied by Spearman. He has shown us that every form of ratiocination can be resolved into simple processes which are of two kinds and two kinds only—the eduction of relations and the eduction of correlates. If we examine one of Euclid's propositions in the light of the new theory, we shall find that it consists of a progressive series of eductions, each eduction, since the relation involved is nearly always a relation of pure equality, being extremely simple in itself, but the whole structure appearing complex on account of its progressive and cumulative nature. The same is true of all mathematical reasoning. In their final analysis all arithmetical and algebraic processes, the highest as well as the lowest, prove to be compounded of the two primary processes of eduction—the eduction of relations and the eduction of correlates.

Illuminating as Spearman's theories are in the realm of logic, it is in the realm of education that they show the most abundant light. There is indeed no educational problem of modern times which has not been clarified if not solved by the methods and principles formulated by Spearman. Let us, for instance, consider the question of mental tests. The testing of intelligence has been pursued with increasing assiduity for the last twenty years. The mere volume of work has assumed colossal dimensions. Yet the bulk of it has been purely empirical. The testers have been fumbling after something vague, undefined, indeterminate. The results have differed in different hands, and nobody has been able to explain the difference.

The reason for all this confusion is not far to seek: there has been no clear hypothesis guiding the experiments, no fixed criteria by which the results may be evaluated and interpreted. Fortunately these strictures do not apply to all the investigations; they do not apply to those conducted under Spearman's guidance. His well-known theory of general and specific factors in our mental make-up (a theory which he first put forward a quarter of a century ago, and which his more recent researches in noegenesis serve to establish) is the only constructive theory of intelligence that has ever been put forward. All other theories, when they are not too nebulous to be of any use, are merely negative and destructive. Under the new analysis of cognition, not only does the general aim of intelligence testing acquire some sort of scientific precision but each particular test takes on a new meaning. There is now some chance of knowing what it is that each test tests. And it is becoming increasingly clear that the connection between noegenesis and intelligence is very close indeed.

Of all the educational problems of modern times, or indeed of all time, the most vital is that of formal training. How far is mental power gained by the exercise of one particular mental function available for use in other functions? To what extent is the result of training transferable? In spite of the fact that an army of researchers has for many years been attacking the problem both in Europe and America, no final solutions have yet been reached. The only conclusion agreed to by all is that the amount of transfer is not so great as we thought it was; and the only explanation of transfer that has been offered is that it is due to the presence of common factors, though nobody can say what the common factors are. The consequence is that of late years the enthusiasm of the researchers has languished almost to the point of extinction. Once more do we find scientific progress checked by the lack of a complete analysis of mental processes. Spearman's new analysis gives to the research a new impetus and a new orientation. The problem of formal training can now be attacked from a different angle, and this time with a reasonable chance of reaching a successful issue.

Let us now take a problem which is more immediately practical. How much memorizing should be done at school? What is its relation to intelligence or reason? If, for the loose and ambiguous words "intelligence" and "reason," we substitute the word eduction, which has now been given a definite scientific meaning, it is easier

to answer the question. We require our pupils to make progress, and Spearman has demonstrated that the only steps by which the mind can march into its new realms of thought are the two eductive processes. Direct experience and memory provide the solid starting-point, but the forward thrust, the life and movement of the human spirit, comes from eduction. As the verb is the soul of the sentence, so is eduction the soul of thinking, and as the verb cannot live without the noun, so does eduction owe its very existence to the tributary service of sense and memory. It is quite impossible to build up in the pupil's mind such a structure of knowledge as is illustrated in the last diagram unless the relations already educed are automatically recalled so as to form the fundaments of new relations. All this has an obvious and important bearing on the rote work of our schools. It indicates when the essential aim should be to foster understanding and when it should be to fix a habit.

The applications of the noegenetic theory are not confined to the academic world; they extend over the whole of human life. In all purposive behavior, in all plotting and planning, in all devising of means to ends, the warp and woof are the two processes of eduction. If the matter be well scrutinized, it will be found that the final step in the solution of a practical problem is always the eduction of a correlate.

I have but touched the fringe of a wide topic. I have traced only a few of the many ramifications of the theory of noegenesis; but I have said enough to show that it possesses the one quality that alone of all others gives a theory the hallmark of scientific validity, that is, it is *fruitful*. Not only does it, in conjunction with Spearman's other cognitive principles which deal with the quantitative side,[3] bring simplicity and order into a chaotic region of pure psychology but it leads to new discoveries in various realms of applied psychology. It solves old problems and gives rise to fresh ones. It thus stimulates research and advances our knowledge of the way in which the mind exercises those functions which are most distinctly human.

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[3]The three noegenetic laws are qualitative only; they need to be supplemented by quantitative laws. For the former merely indicate a "tendency" whereas the latter indicate when and where the tendency is realized.

## References

1. Spearman, C. E. The abilities of man: their nature and measurement. New York: Macmillan, 1927. Pp. x+415+xxxiii.
2. ————. The nature of "intelligence" and the principles of cognition. London: Macmillan, 1923. Pp. viii+358.

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## LA NOEGÉNÈSE

(Résumé)

La noegénèse est un mot employé par le professeur Spearman pour signifier le moyen par lequel les connaissances nouvelles entrent dans l'esprit humain. Ecartant la classification traditionnelle des processus mentaux il a essayé de trouver les facteurs de l'esprit qui one une vraie unité fonctionnelle. Au moyen de tests, de mesures, et d'analyses statistiques, il a trouvé les éléments définitifs et les simples lois fondamentales. Ses éléments sont des *caractères* et des *relations,* et ses lois sont de deux sortes, qualitatives et quantitatives. De ces lois, les qualitatives représentent les contributions spéciales de Spearman à la théorie de la cognition. Ce sont les lois de la Noegénèse et sont trois en nombre.

La première est approximativement ceci: *"Toute expérience vécue tend à évoquer tout de suite la connaissance de ses caractères et de ses expériences.*

La deuxième, appelée l'éduction de relations, est ceci: *"La présentation de n'importe quels deux caractères ou plus tend à évoquer tout de suite la connaissance de la relation entre eux."*

La troisième s'appelle l'éduction des corrélations: *"La présentation de n'importe quel caractère avec une relation tend à évoquer tout de suite la connaissance du caractère corrélatif."*

Le processus de l'éduction a une grande importance pour tous les actes de cognition qui ne sont pas simplement reproductifs. C'est un facteur essentiel de l'intelligence génerale, c'est la base de tout raisonnement, et il joue un rôle dans toutes les formes du comportement avec intention.

Ballard

## NOEGENESE

(Referat)

Noegenese ist ein Wort, dass Professor Spearman anwendet die Art und Weise zu bezeichnen wie neues Wissen in den Menschengeist eindringt. Er bricht mit der traditionellen Anordnung geistiger Vorgänge und versucht die Faktoren im Geist zu ergründen, die wirkliche funktionelle Einheit besitzen. Durch Tests, Messungen und Analysen hat er die endlichen Elemente und einfache Grundgesetze gefunden. Seine Elemente sind *Charakter* und *Beziehungen,* und seine Gesetze sind zweierlei Art: qualitativ und

quantitativ. Von diesen stellen die qualitativen Professor Spearmans besondern Beitrag zu der Theorie der Erkentnis vor. Es sind die Gesetze der Noegenese, drei an der Zahl.

Das erste Gesetz lautet etwa wie folgt: *"Irgend ein Erlebnis bedingt sofort eine Kenntnis des Charakters des Erlebten und dessen der es erlebt hat."*

Das zweite, die Ableitung (Eduction) der Beziehungen genannt, lautet: *"Die Darstellung von zwei oder mehr Charakteren bedingt sofort eine Kenntnis der Beziehungen zwischen ihnen."*

Das dritte Gesetz heisst die Ableitung der Korrelate und lautet wie folgt: *"Die Darstellung irgend eines Charakters mit einer Beziehung bedingt sofort eine Kenntnis des korrelativen Charakters."*

Der Vorgang der Ableitung ist von höchster Bedeutung in jedem Akt des Erkennens in so fern ein solcher nicht rein reproduktiv ist. Es ist ein wesentlicher Faktor in der allgemeinen Intelligenz, es ist die Wurzel allen logischen Denkens, und wirkt in jeder Art des zielbewussten Verhaltens.

BALLARD

## НОУГЕНЕЗИС.

(Реферат).

Ноугенезис (Noegenesis)—термин, используемый проф. Спирменом (Spearman) с целью показать, каким способом новое знание включается в состав человеческой мысли. Отбрасывая традиционную классификацию умственных процессов, он сделал попытки установить, какие умственные факторы обладают действительным функциональным единством. При помощи тестов, измерений и статистических анализов он пришел к неразложимым далее элементам и простым основным законам. Его элементы—характерные признаки и отношения, его законы—двух родов, качественные и количественные. На качественные законы и опираются специальные исследования Спирмена по теории познавательной способности. Они (числом—три) и являются законами ноугенезиса.

Первый закон звучит приблизительно так: „Каждый пережитый опыт имеет тенденцию непосредственно вызывать узнавание его характерных признаков".

Второй закон, который называется законом образования отношений, выражается следующим образом: „Пред'явление любых двух или большего числа признаков имеет тенденцию непосредственно вызывать восприятие отношения между ними".

Третий закон называется законом образования коррелативных признаков и заключается в следующем: „Восприятие любого признака вместе с известным отношением имеет тенденцию непосредственно вызывать узнавание коррелативного признака".

Процесс вывода („эдукции") имеет кардинальное значение во всех актах познавания, не являющихся простым воспроизведением. Он представляет собой существенный фактор мыслительной деятельности, он лежит в основе всякого рассуждения, он включается во все формы целенаправленного поведения.

**Боллард (Ballard).**

# DERMAL SENSITIVITY AND THE DIFFERENTIATED NERVE-TERMINATIONS OF THE HUMAN SKIN*[1]

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## I. EVIDENCE FOR SPECIFICITY OF FUNCTION

The fact that a relationship exists between the distribution of certain structurally differentiated nerve terminations and cutaneous sensitivity to specific forms of stimulation furnishes the basis for our well-known theories of specificity. Such relationships have been interpreted as indicating that each type of corpuscular or free nerve-ending serves as the receptive end-organ for a specific sense modality, being specialized for the reception of a particular form of stimulation. Trite though it may seem to those who are familiar with the literature in the field of cutaneous sensitivity, it will perhaps prove worth while to review some important facts before proceeding to the present problem.

A. *Tactile Receptors.* Meissner (14) counted in an area two and two tenths millimeters square on the third phalanx of the index finger 400 papillae, and observed tactile corpuscles (Meissner corpuscles) in 108 of them. In equal areas he found 40 corpuscles on the second phalanx, 15 on the first phalanx, 8 on the skin of the hypothenar eminence, 34 on the plantar surface of the ungual phalanx of the great toe, and seven or eight on the middle of the sole of the foot.

Shäfer (19) in his *Textbook of Physiology* presents the following figures as the average number of Meissner corpuscles in one square millimeter of skin surface:

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[1]The original paper of which this is a part contains a discussion of the microscopic anatomy of the skin, a description of the specialized nerve endings in the skin, a history of their discovery, and a list of some 200 books and articles on this subject. On request it may be borrowed from the Department of Psychology of Clark University.

| | | |
|---|---|---|
| | End phalanx of index finger | 21 |
| | Second phalanx of index finger | 8 |
| Volar surface of | Proximal phalanx of index finger | 4 |
| | Metacarpus of little finger | 2 |
| | Plantar face of end phalanx of hallux | 7 |
| | Middle of sole of foot | 2 |

It is a noteworthy fact that Meissner corpuscles are most numerous in regions that are most sensitive to tactile stimuli, that is, where the two-point threshold is smallest. The two-point threshold on the tip of the index finger, where Meissner corpuscles are most numerous, is smaller than on any bodily region with the exception of the tip of the tongue.

Pressure spots have been reported by Goldscheider as having a definite relation to the hairs, there being one or more near the emergence of each hair, usually on the side from which the hair slopes—the so-called "windward" side. The nerve fibers which arborize about the hair follicles have come to be quite generally regarded as the receptor mechanisms for tactile stimuli in the hairy regions, being homologous with the Meissner corpuscles of the hairless areas. In the cornea, conjunctiva of the upper lid, and in the glans penis, no touch spots can be found; likewise there can be found neither hair follicles nor Meissner corpuscles in these parts.

B. *Temperature Receptors.* A considerable amount of evidence has been gathered which indicates that the Krause bulbs are the specific receptors for cold stimuli in the regions where they are most numerous, i.e., the periphery of the cornea, the conjunctiva, and the glans penis. However, it has not been shown that these structures exist in other regions in numbers sufficient to account for cold sensitivity.

Much of the work on the temperature sensitivity of the cornea and conjunctiva has been done by, or under the direction of, M. von Frey (8). Von Frey admits that he at one time published the affirmation that the cornea and conjunctiva lack the temperature sense completely. However, stimulated by the papers of Fuchs (9) and Molter (15), in which it was claimed that these surfaces are very sensitive to thermal stimulation, and by similar statements by Donaldson (7) and Dessoir (4), he undertook more thorough investigations. Von Frey was convinced after completing his first series of experiments that, while he could not agree with these investigators in all their statements, his own affirmations should be qualified (8). Thereupon followed a long series of investigations by him and his students.

Von Frey mapped out the cold spots on 21 square millimeters of his own right conjunctival bulb, and found there ten cold spots and 35 pain spots. He states that some cold spots lie very near pain spots, while others are isolated, and that many cold spots are found along the conjunctival blood vessels.

On the cornea all trials produced negative results, excepting at the periphery, within a millimeter or two of the edge.

Von Frey also explored the glans penis, and found the temperature sensitivity of this region to be preponderately cold, and that from the pads of the glans to the urethra the sensitivity to thermal stimulation falls off rapidly. The cold spots of the glans react clearly to faradic stimulation. The threshold for thermal sensitivity is higher than that for pain sensitivity. The cold spots of the glans answer easily to paradoxical stimuli, the cold sensation increasing in proportion to the excess of the stimulating temperature. Aside from temperature and pain, von Frey finds no other form of sensitivity in the glans.

For the human body von Frey concludes from the results of this and of previous work that in a given area warm spots are most sparse, that there are more cold spots, a yet greater number of pressure spots, and that pain spots are the most numerous of all. His summary of the cutaneous regions of the body is as follows:

1) Areas sensitive exclusively to pain.
   *a*) Cornea with the exception of its periphery.
   *b*) The teeth, particularly the dentine and pulp.
2)
   *a*) Regions sensitive to pain and temperature.
      (1) The periphery of the cornea and the conjunctiva.
      (2) The glans penis.
   *b*) Regions with pressure and temperature sensitivity.
      (1) The mouth.
3) All other regions sensitive to temperature, pressure, and pain.

Von Frey, in the same article, discusses the relationship of the distribution of the specialized nerve endings to the distribution of sense qualities. The substance of his discussion is as follows:

1) Only free nerve-endings are to be found in the cornea, excepting at its borders. This corresponds to the exclusive presence there of sensitivity to pain. Free nerve-endings are also found in the epithelium of the conjunctiva, in the skin of all other regions, and in many mucous membranes. The endings in the teeth are not yet well known (1895), and we do not know if the innervation of the mouth corresponds with the poverty of pain sensitivity, says von Frey.

The pain nerves lie near the surface of the skin, and von Frey claims that in all body surfaces which are covered by a thin epithelial layer the threshold for electrical stimulation of the pain spots is lower than that for all the other sensitive spots.

2) Von Frey adds that end bulbs have been found by Krause (13) in the conjunctiva, in various parts of the mouth, the glans penis, and in the clitoris. He thinks that their presence in the conjunctiva and glans, where pressure sensitivity is lacking, shows them to be the organs of the temperature sense. Dogiel (6) shows that they are very thickly distributed in the periphery of the cornea, but there warmth sensitivity is absent. He concludes then, that the end bulbs of Krause are probably the end-organs of cold sensitivity—"Die Endkolben sind daher wahrscheinlich die organe der Kaltempfindung."

Von Frey admits that this assumption seems to present a real difficulty in that cold sensitivity is characteristic not only of the parts discussed above but of others as well. But he claims that end bulbs have been observed in other regions also, Smirnow (20) having observed them in the skin of the sole of the foot. Ruffini (17) found in the skin of the fingertip corpuscles which he identified with the corpuscles of Golgi, but which von Frey claims can, with as much authority, be regarded as Krause end bulbs. That they lie deeper here than in other places is not much of an objection, says von Frey, as the finger pad shows a peculiar behavior towards cold stimuli. Kiesow states, and von Frey verifies the statement, that contact with the point of a metal cone is not perceived as thermal excitation: to initiate cold sensation it is necessary to apply a larger surface. Between the deep situation here and the superficial one in the glans penis there are intermediate steps, he claims.

Von Frey claims to have found, in a gold preparation, a true end bulb in the skin of the upper arm, three tenths of a millimeter under the epithelium, about half way between the knots of the sweat glands and the surface. He admits that he does not know whether or not the number of end bulbs in the skin is sufficient to account for the number of cold spots.

3) Von Frey speaks of the "new nerve ending" (1895) of deeper position, larger dimensions, and cylindrical form, discovered by Ruffini. He believes that it may well be the specific receptor for warm stimuli, since there are various reasons for believing that the warmth receptor is larger and that it lies deeper than the cold receptor. All investigators agree that the clear determination of the

warm spots is much more difficult than that of the cold spots. It is seldom that sharply defined warm spots are found. Intense warm stimuli are perceived over almost all of the body, although with varying clearness and localization. Von Frey also suggests that the increased size of the supposed warmth receptors may account for their smaller number. The longer reaction time noted by Herzen (11) and checked upon by many other investigators is probably due to the depth at which they lie.

Since the appearance of von Frey's publication of 1895, the exploration of the cornea and the conjunctiva has been repeated, checked upon, and continued by von Frey himself and by others. In 1925 Strughold and Karbe (22) published a series of reports on work of this nature. They attempted to make a new topography of the cold sensitivity of the whole eye and to compare the findings with anatomical findings concerning Krause bulbs.

On the cornea, sensitivity to cold was found to be greater along the horizontal diameter than along the vertical. Thus the area in the center of the cornea which is insensitive to cold has the form of an ellipse whose shorter diameter lies horizontally. Comparing this topography with the distribution of Krause bulbs, the authors mentioned above find that in the central part of the cornea, which is insensitive to cold, no Krause bulbs have ever been observed by any of the following: H. Frey, Arnold, Ludden, Hauchle, Helfreich, Stricker, Ciaccio, Poncet, Longworth, Waldeyer, Key and Retzius, Merkel, Suchard, Kolliker, Dogiel, Attias, or Knusel and Vonwiller. (The references to the works of these investigators are presented on page 195 of the report of Strughold and Karbe (22) ). Only fine intra-epithelial free nerve-endings have been observed in this central area of the cornea. Dogiel (5) and Attias (1) show that there are many Krause bulbs in the periphery of the cornea.

In a later article Strughold (21) published data on the threshold of cold sensitivity in the eye. He found that in the cornea and conjunctiva the threshold is highest where the Krause bulbs lie deepest beneath the surface, and lowest where they lie just beneath the surface.

Again, Strughold and Karbe (23) show that the greatest sensitivity to cold in these regions is found where Krause bulbs are most numerous.

One of the most interesting of the recent investigations is that conducted by these same investigators, in which they were able to

observe Krause bulbs in the living tissue of the eye (24). Using the method of Knusel and Vonwiller (12), methylene blue was injected into the sclerotic membrane of the living eye. Krause bulbs were thus made visible with the aid of the corneal microscope. An attempt was then made to stimulate the bulbs individually by applying cold stimuli, the classic copper wire cylinders being used as stimulating instruments.

Two Krause bulbs were made visible in one of the eyes thus treated, and a group of four in another. As cocaine was used before the injection of methylene blue, it is not surprising that the application of a cold point immediately over the visible bulbs evoked no sensation. However, the exact location of the bulbs was determined, with reference to some of the small superficial blood-vessels, so that stimulation of the same points might be possible after the effects of the cocaine had disappeared, even though the color would have disappeared also. The color of the stained nerves faded after ten minutes, which was somewhat later than the fading of the epithelial cells. The anaesthesia remained for about two days. On the third day the spots corresponding to the bulbs reacted to an intense cold stimulus with a weak cold sensation. After eight or ten days a definite, clear, cold sensation was always called forth by weak stimuli on these spots, while the immediately adjacent and surrounding areas remained insensitive to cold. While these investigators did not stain and subsequently test a large number of end bulbs, they believe it to be demonstrated that the Krause bulb is the specific receptor for cold stimulation.

## II. Some Negative Evidence for Specificity of Function

Interesting observations by Goldscheider make the assumption that the temperature receptors of the hairy regions are of a corpuscular nature, as von Frey supposes, appear rather doubtful. Goldscheider (10) has carefully marked temperature and pressure spots on his own left forearm, extirpated the portion of dermal tissue containing these sensitive points, impregnated them with gold chloride, sectioned them, and made a careful microscopical study of them. He found beneath the sensitive dermal points designated as pressure spots, cold spots and warm spots, not sensory nerve-terminations of the corpuscular type, but different forms of arborized free nerve-endings. However, the terminal arborizations were not of the same kind for the three types of sensitive points.

Goldscheider describes the innervation of the pressure spots somewhat as follows: a bundle of nerve-fibers emerges from the deeper layers of the dermis, more or less obliquely with respect to the epithelial layers. Just before reaching the epithelial cell layer the trunk splits up in a number of nerve-fibers, which spread out flatly immediately under the epithelial layer. Each of these fibers proceeds for a certain distance, meandering slightly, close to the stratum mucosum, while now and then they send up against the latter fine branches which finally, with a slight upward bending, reach the deepest cell layer, in which they find their terminations. It is not clear from Goldscheider's description that these terminal fibers end between the cells of the Malpighian layer, but we are told by other investigators that such is the case (2).

Goldscheider found it impossible to draw any satisfactory anatomical distinction between the nerve-terminations corresponding to warm spots and cold spots. As in the case of the pressure spots, a nerve ascends obliquely upwards in the cutis and proceeds directly to the location of the "Stichkanal," which is a conical hole made by the insertion of a needle, perpendicular to the surface of the skin at the point on the surface marked before extirpation. In the papillary layer these nerves resolve themselves into a number of fine fibers which do not spread out under the epithelium to any great extent, as in the case of the pressure spots, but grow up in short courses, obliquely directed towards the epithelium, forming a sort of umbel. These branch, but the ramification develops somewhat deeper in the cutis than does that of the pressure nerve. The terminal fibers proceed to the border of the Malpighian layer, some of them reaching the latter, while some terminate in the most superficial layer of the cutis. The ramifications of the innervation of cold spots are always found to lie in a striking immediate proximity to tufts of blood-vessels. The end branchings of these temperature nerves are very delicate and short in comparison with the corresponding ones of the pressure nerves. Furthermore, the nerve-trunk to the pressure point is larger than that to the temperature point. The delicacy of the terminal fibers together with their intimate association with the blood-vessels makes the histological investigation of temperature spots rather difficult.

Goldscheider suggests that the intimate relationship of the temperature nerves to the blood-vessels raises the question as to whether the vessel is perhaps an integral part of the end-apparatus of tem-

perature sensitivity. The fact of the constricting of the vessels with the cooling and the dilating with the warming suggests that it quite likely functions as a stimulus-mediating agent. At cold spots, contraction may be the adequate stimulus for the nerve, while at warm spots dilation is the stimulus. Thus the temperature nerve would be a kind of centripetal blood-vessel nerve. Here Goldscheider thinks that a definite calibre of the blood-vessel would be necessary for a particular temperature, perhaps as in the case of mercury in the thermometer. We have been accustomed to think of the change in the content of the vessels as initiated by the preceding nerve processes, however, and it would seem that the fact that the nerve-fibers project beyond the vessels and into the epithelium speaks decidedly against the centripetal character of blood-vessel nerves. In other words, if the blood-vessels are the end-organs, why do the nerve-fibers extend beyond the vessels and into the epithelium? Goldscheider believes rather that the vessels are placed in close proximity to the terminal temperature apparatus in order that the former may serve to bring the temperature of the latter back to normal quickly after hot or cold stimulation. The vessels furnish, he thinks, an efficacious compensation for injurious temperatures.

As mentioned above, Goldscheider did not find, as he had hoped to do, an anatomical differentiation between the two kinds of temperature receptors, warm and cold. He states, however, that menthol acts chiefly on the nerves for cold sensitivity, and that it is much less irritating for those for warmth sensitivity. Herz is quoted as stating that in the case of pressure exerted upon nerve-trunks, as when the limbs "go to sleep," the cold sensitivity disappears first and, much later, the warmth sensitivity.

It is a noteworthy fact that Goldscheider found no tactile corpuscles beneath the pressure spots which he had marked and extirpated. He believes it to be true beyond a doubt that the so-called tactile corpuscles stand in direct relationship with the sense of touch, for they are most numerous in the especially sensitive tactile areas of the body, i.e., the fingertips, palms, and the soles of the feet. The question is, however, relative to the nature of this relationship. The hand is sensitive to the finest and most delicate of mechanical stimuli; yet it is at the same time exposed to the most violent of shocks and the heaviest of pressures. Think of the pressure to which the subepithelial nerve terminations in the hands of laborers and gymnasts are exposed; yet they remain sensitive. The corneal layer

is much thicker in the palms of the hands and the soles of the feet than in any other region of the body, thus furnishing the necessary protection for these parts. May it not be that the corpuscles do not function as end-organs, but rather serve as an additional protection for the nerve-endings in these regions? Goldscheider cites the equal distribution of corpuscles in the hands and feet of apes as indicating that they do not bear a relationship with the tactile sensitivity which we have acquired in the hands, but rather with the use of the extremities for work. Furthermore, the number of corpuscles does not correspond to tactile sensitivity in every instance; for example, fewer corpuscles are found in the tip of the tongue than in the fingertip, although tactile sensitivity is greater in the tongue than in the fingertip. We might add to Goldscheider's evidence another point. Histologists describe many corpuscles, some of a quite specialized type, which are to be found in the bill of the duck, in the bills of other birds, and in the snout of the pig. The bills and snouts of animals are members which, so far as we know, do not possess extremely delicate tactile sensitivity, but they are subjected to a great deal of wear and tear and pressure of the kind which we might reasonably suppose might call for protective sheaths for the nerve-terminations. Why, then, we may ask, are there in the hands and feet some sensory nerve-terminations which have no protective capsules? The answer may be that these endings belong to modalities which have higher thresholds of sensitivity, and which need not be so highly developed and delicate as are the finely discriminating tactile terminations. Thus they do not need the protection of a capsule. It is a known fact that the threshold of pain sensitivity is higher than that for the other modalities. It may very well be that the more elaborate terminal apparatus of nerve-fibers, which is characteristic of those which are encased in capsules, is demanded as a compensation for the greater resistance offered by the protective sheaths.

Goldscheider's extirpation experiment was repeated some years ago by Dallenbach, with the result that no corpuscular nerve-terminations were found beneath the sensitive points marked.

## III. The Present Problem

It would seem that, with a few exceptions, the assumption of functional receptor specificity is based largely upon speculation and upon experimental data derived from experimentation upon limited areas of the human body, viz., the cornea and conjunctiva of the

eye, the glans penis, and the mouth. A considerable amount of evidence has been gathered in support of the view that in these regions the Krause end bulb functions as the specific cold receptor. Especially in the eye does this seem to be the case. This has led to the generalization that the Krause bulb is the end-organ for cold sensitivity, implying that it functions as such throughout the body, or in all regions where cold spots exist. The fact that the Ruffini corpuscle lies at a deeper level in the skin, coupled with the fact that the threshold for warmth sensitivity is considerably higher than that for cold sensitivity, has led to the conjecture that this particular corpuscle functions as the specific receptor for warm stimuli. There is more evidence for the existence of a definite relationship between tactile sensitivity and Meissner corpuscles and hair follicle innervations, although, as Goldscheider has pointed out, there are reasons for believing that the anatomical features which cause the former to be placed in the corpuscle category are not integral parts of the receptor apparatus, but that they function merely as supporting and protective structures. According to this view, corpuscles as such are not receptors.

The aim of the present work is to examine microscopically samples of skin taken from several regions of the human body in order to study the distribution of terminal corpuscles in general, and to see if certain of them exist in numbers sufficient to account for the modalities of sensitivity with which they have been associated.

A. *Material.* Samples of human dermal tissues from seven bodily regions were examined, these regions being determined largely by what tissue medical examiners and surgeons were able to obtain for us. They were as follows: thigh along the sartorius muscle, abdominal epigastric region, left breast at the nipple, left breast below the nipple, middle of the sole of the foot, chest, leg along the tibia. As much subcutaneous tissue as possible was taken along with the dermis, and care was taken to select normal tissue in every case. The specimens were all in good condition when obtained for sectioning, having been kept but a short time, and in a refrigerator, and in many cases it was possible to place the tissue in fixing fluid immediatley upon the removal from the donor. A fairly large piece of tissue was taken in most cases, being sometimes as large as two and one half by five or six centimeters. These were placed immediately in a 10% solution of formalin, which, in addition to preserving the tissue, acts as a very satisfactory killing and fixing

fluid. In some cases Zenker's fluid was used, but with less success for our purposes.

B. *Method.* Tissue preserved in formalin was cut into pieces from two to five millimeters in width and five millimeters to one centimeter in length for sectioning. These small pieces were allowed to remain in 10% formalin for several hours to insure thorough fixation, which might have been incomplete in the undivided pieces of tissue. As we found it impracticable to follow any prescribed technique closely, our process of hardening will be described.

From the formalin the tissue was placed without washing in 15% alcohol, and then up through a graded series of alcohols; 25%, 35%, 50%, 85%, 95%, and absolute, remaining in each alcohol solution for a period of from 12 to 24 hours, a much longer time than is usually prescribed by handbooks of histological technique. This process dehydrates and at the same time hardens the tissue. From the absolute alcohol the tissue was placed first in a mixture of xylol and absolute alcohol in the proportion of one part of the former to three of the latter, then in a half and half mixture of the same, next in a mixture of one part absolute alcohol and three of xylol, and finally in clear xylol, remaining in each of the mixtures from 12 to 24 hours. When cleared, the tissue was placed in a solution of paraffin in xylol for infiltration. Infiltration in melted paraffin in the paraffin oven followed, the tissue usually remaining in the oven about 12 hours. In order to obtain good sections it was found necessary to lengthen the period of fixing in the graded alcohols and the period of infiltration considerably over the time specified by most manuals of histological technique.

The tissue was imbedded in paraffin and serial sections were made with a rotary microtome. Staining was by the so-called "ordinary" stains. Delafield's haematoxylin was used as a nuclear stain and eosin as a counter stain. In some cases acid fuchsin was employed as the counter stain, but on the whole eosin was found to produce more satisfactory results.

The sections were cut 10 and 12 micra in thickness, and at right angles to the long axis of the rectangular pieces, the microtome knife being made to strike the subcutaneous tissue first and the corneal layer last. In this way folding of the corneum over the deeper layers was prevented. When arranged in serial order on the slides, the sections assumed a uniform position in all the series; the surface of the skin (corneum) was always at the right, appear-

ing at the left when viewed under the microscope. The number of sections per slide averaged about 35, so that a set of 12 or 15 slides represented a strip of tissue five and a half or six millimeters in length. The blocks of tissue varied between three and five millimeters in width, so that the area covered by the average set of 12 or 15 slides varied between 18 and 30 square millimeters. A few of the series were much shorter, however.

The use of ordinary stains was not decided upon until many trials had been made. Our original plan was to make use of special nerve stains, so that the nervous tissue of the corpuscles might be differentiated, but after trying several it was decided that such stains are too capricious to enable us to obtain quantitative data. It is extremely difficult to obtain a complete series of sections of tissue stained or impregnated with nerve stains. We found, however, that corpuscular nerve terminations can de detected easily through the use of haematoxylin and eosin, even though these stains do not enable these structures to be studied in detail. The nervous tissue elements are not distinctly differentiated, but the connective tissue of the capsules is rendered quite distinct, as demonstrated not only by our own preparations, but also by those of Dr. Thyng and Dr. Sykes of Tuft's Medical School, in which Pacinian corpuscles were easily identified in the subcutaneous tissue of the index finger.

In order to test our method for its efficiency in differentiating the connective tissue of the corpuscles, the tissue taken from the sole of the foot was searched first, as there are known to be Meissner corpuscles in this region. Three series were examined, comprising a total of about 900 sections, representing a skin area of about 32 square millimeters. The middle of the sole of the foot is known to contain but few Meissner corpuscles as compared with the finger tips, palms, or even the toes, but within the area examined we were able to observe two bodies which were unmistakably Meissner corpuscles, well differentiated from the surrounding tissue. All parts of the corpuscles were plainly visible with the exception of the coils of the nerve-fiber. Their dimensions were in the one case 80 micra long by 40 wide, while in the other they were 96 by 48 micra. Ruffini (19), in his summary of work on sensory nerve-terminations, gives the dimensions as varying between 30 and 108 micra in length and 30 and 100 micra in width, so that our measurements are seen to correspond and may be taken as one check upon our method.

Having satisfied ourselves that corpuscles were being differentiated sufficiently by our stains, we proceeded to search through our whole stock of sectioned tissue for any terminal corpuscles. Having observed Meissner and Pacinian corpuscles in good haematoxylin-eosin sections, we see no reason why, if the sheaths of these corpuscles can be differentiated so easily, we should not also be able to detect the presence of other forms which differ principally in size and shape.

The inspection of the sections was made as systematically as was possible. They were taken in their serial order and scrutinized with both low and high power objectives. Bausch and Lomb 16mm. and 4mm. objectives were used with 5X and 10X oculars of the same make, affording magnifications of 50, 100, 215, and 400 diameters. The following series were studied, including the foot tissue already mentioned.

| | Series | Slides | Sections | Approximate Area |
|---|---|---|---|---|
| 1) Sole of foot | 1 | 14 | 530 | 19.08 sq. mm. |
| | 2 | 5 | 206 | 7.41 |
| | 3 | 4 | 157 | 5.65 |
| 2) Thigh | 1 | 15 | 640 | 23.04 |
| | 2 | 15 | 530 | 19.08 |
| | 3 | 9 | 384 | 13.82 |
| | 4 | 22 | 375 | 13.50 |
| 3) Leg | 1 | 18 | 620 | 22.32 |
| 4) Epigastrium | 1 | 8 | 240 | 8.64 |
| | 2 | 14 | 430 | 15.48 |
| 5) Chest | 1 | 11 | 430 | 15.48 |
| 6) Breast below nipple | 1 | 11 | 425 | 15.30 |
| 7) Breast at nipple | 1 | 10 | 310 | 11.16 |
| | | | 5277 | 189.96 sq. mm. |

In all, 5277 sections were examined, representing approximately 190 square millimeters of skin surface.

C. *Observations.* In the hairless tissue of the sole of the foot Meissner corpuscles were observed with great facility, but no other types of corpuscles were found there.

In none of the 4384 sections of skin taken from hairy regions of the body were terminal corpuscles of any kind encountered. Our sections did not always include the whole depth of the subcutaneous tissue, making it impossible to state whether or not there were Pacinian corpuscles in these regions. However, in all cases we were able to examine thoroughly the whole of the chorium and the epider-

mis. Especial attention was given the papillary layer and the tissue immediately below it, where the Krause bulbs, the alleged end-organs for cold sensitivity, are supposed to lie. However, no Krause bulbs were found.

D. *The Significance of the Facts Observed.* The apparent absence of differentiated nerve-terminations in the form of corpuscles throughout large areas of the hairy regions of the human body makes certain theories of specificity appear doubtful. We have admitted that the Meissner corpuscles and the hair follicles may be the end-organs for superficial light pressure or tactile sensitivity. But the dearth of corpuscles of any kind in some of the hairy regions makes it impossible for us to believe that the Krause bulb or any other corpuscular termination is the specific cold receptor, or that the Ruffini corpuscle is the specific warmth receptor. Our findings are in agreement with those of Pendleton, who hunted for end bulbs in the hairy regions of the skin, using non-haematoxylin stains, and with those of Dallenbach, who repeated some of Goldscheider's work, as described above. Reports of the work of Pendleton and Dallenbach are as yet unpublished.[2]

Pendleton, in a private communication, quotes Ruffini as claiming that no Krause bulbs have ever been found in the hairless parts of the hands and feet, though he does not give the specific reference. He states that von Frey assumes that the Golgi-Mazzoni bulbs are forms of the Vater-Pacini corpuscles, which have been found in the hairless portions of the hand and foot, in tendons, mesentery, and other places, but we find no mention of anyone finding them in the hairy parts of the human body, with one exception—von Frey found one in the arm, as noted in the previous discussion of his work. As Pendleton points out, if they were present they should be easily visible, because they are 50 micra long, they are surrounded by a distinct sheath, and they can be seen in many sections of the hairless parts of the body.

## REFERENCES

1. Attias, G. Die Nerven der Hornhaut des Menschen. *Arch. f. Ophth.*, 1912, **83**, 207-317.
2. Cajal, R. Textura del sistema nerviso del hombre y de los verterbrados. Vol. 1. Madrid, 1899. P. 384. Histologie du system nerveux de l'homme et des vertebres. Vol. 1. Paris, 1909. P. 464.

[2]Since this article was accepted for publication, the experimental findings of Pendleton (16) and Dallenbach (3), referred to above, have been published.

3. DALLENBACH, K. The temperature spots and end-organs. *Amer. J. Psychol.*, 1927, **39**, 402-427.
4. DESSOIR, M. *DuBois-Reymond's Arch.*, 1892, 175.
5. DOGIEL, A. S. Die Nerven der Hornhaut des Menschen. *Anat. Anz.*, 1890, **5**, 483.
6. ————. Die Nervenendkörperchen in der Cornea und Conjunctiva Balbi des Menschen. *Arch. f. mikr. Anat.*, 1891, **37**, 602.
7. DONALDSON, H. H. *Mind*, 1885, **10**, 339.
8. FREY, M. VON. Beiträge zur Sinnesphysiologie der Haut. *Ber. d. sächs. Akad. d. Wiss., math.-phys. Kl.*, Leipzig, Sitzung vom 4. März, 1895.
9. FUCHS, E. *Wien med. Jahrb.*, **4**.
10. GOLDSCHEIDER, A. Histologische Untersuchungen über die Endigungsweise der Hautsinnesnerven beim Menschen. *Arch. f. Anat. u. Physiol.*, 1886. *Physiol. Abt. Suppl.* Gessam. Abh., 1. Bd., Physiologie der Hautsinnesnerven. Leipzig, 1898.
11. HERZEN, —. *Arch. f. d. ges. Physiol.*, **38**, 93.
12. KNUSEL, —, & VONWILLER, —. Vitale Farbung am menschlichen Auge. *Zsch. f. Augenhk.*, 1922, **49**, 157—.
13. KRAUSE, —. Die terminalen Körperchen der einfach sensiblen Nerven. Hannover, 1860.
14. MEISSNER, G. Beiträge zur Anatomie und Physiologie der Haut. Leipzig, 1853. Pp. 28. *Zsch. f. rat. Med.*, N. F., **4**, 270.
15. MOLTER, A. (Dissertation, Erlangen.) Cassel: Th. Fischer, 1878.
16. PENDLETON, C. R. The cold receptor. *Amer. J. Psychol.*, 1928, **40**, 353-371.
17. RUFFINI, A. *Arch. ital. de biol.*, **21**, 249.
18. ————. *Rev. gén. d'histol.*, 1904-1905.
19. SCHÄFER, E. A. Textbook of physiology. 2 vols. Edinburgh & London: Pentland, 1898-1900.
20. SMIRNOW, —. *Int. Monatssch. f. Anat. u. Physiol.*, 1893, **10**, 241.
21. STRUGHOLD, H. Die Schwellen des Kaltsinnes am Auge, bestimmt mit Reizen von kleiner Fläche und geringer Wärmekapazität. *Zsch. f. Biol.*, 1925, **83**, 201-206.
22. STRUGHOLD, H., & KARBE, M. Die Topographie des Kaltsinnes auf Cornea und Conjunctiva, ein Beitrag zur Frage nach den spezifischen Empfängern desselben. *Zsch. f. Biol.*, 1925, **83**, 189-200.
23. ————. Die Dichte der Kaltpunkte im Lidspaltenbereiche des Auges. *Zsch. f. Biol.*, 1925, **83**, 207-212.
24. ————. Vitale Farbung des Auges und experimentelle Untersuchung der gefärbten Nervenelemente. *Zsch. f. Biol.*, 1925, **83**, 297-308.

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## LA SENSITIVITÉ DU DERME ET LES BOUTS DIFFERENCIÉS DES NERFS DE LA PEAU

(Résumé)

L'auteur passe en revue l'évidence positive et négative pour les corpuscules des nerfs comme les organes de bout de la sensitivité du derme et ensuite présente son propre travail qu'on est obligé de considérer comme évidence négative.

Cette étude est un rapport de l'examen histologique de 5277 sections représentant 190 mm. carrés de la surface de la peau de treize parties du corps. On a employé l'hématoxyline de Delafield comme teinte nucléaire et l'éosine (quelquefois acide fuchsine) comme teinte de contrôle.

Le tissu de la plante du pied a contenu des corpuscules Meissner mais dans les tissus des parties couvertes de poil on n'a pas trouvé de bouts spéciaux. Une revue des données d'autres observateurs ne montre aucune différence à l'exception que von Frey a trouvé *un* corpuscule Dogiel dans la peau du bras.

On prêtera toute l'étude, qui contient une discussion beaucoup plus détaillée que celle de ce résumé, si l'on s'adresse aux Laboratoires Psychologiques, Clark University.

GILBERT

## ÜBER DIE EMPFINDLICHKEIT DER HAUT UND DIE DIFFERENZIIERTEN NERVENENDIGUNGEN DER HAUT

(Referat)

Der Vefasser führt die Beweise für und gegen die Theorie an, dass die Nervenkörperchen die Endorgane der Hautempfindlichkeit bilden, und berichtet dann seine eigne Arbeit, die man als negativen Beweis hierfür betrachten muss.

Die vorliegende Abhandlung berichtet die histologische Untersuchung von 5277 Schnitten, die ungefähr 190 quadrat mm. der Hautoberfläche verschiedener Körperteile entnommen waren. Man benutzte Hämatoxylinlösung (Delafields') als Kernfarbe und Eosin (mitunter saures Fuchsin) als Gegenfarbe.

Das Bindegewebe der Fuszsolen enthielt Meissnersche Körperchen, doch in dem Bindegewebe behaarter Teile fanden sich keine besondern Endigungen irgendwelcher Art. Die Befunde anderer Forscher stimmen hiermit überein, mit der einzigen Ausnahme dass von Frey *ein* Dogielsches Körperchen in der Haut des Armes fand. Die Original Abhandlung, die eine viel Gründlichere Erörterung des Problems enthält, wird auf Anfrage im psychologischen Laboratorium der Clark Universität gern geliehen werden.

GILBERT

## КОЖНАЯ ЧУВСТВИТЕЛЬНОСТЬ И ДИФЕРЕНЦИРОВАННЫЕ НЕРВНЫЕ ОКОНЧАНИЯ КОЖИ.

(Реферат).

Автор дает обзор положительных и отрицательных данных о нервных тельцах, как концевых органах кожной чувствительности, и затем излагает свой собственный труд, который должен подкрепить отрицательные данные по этой проблеме.

Работа представляет собой сообщение о 5277 гистологических срезах, охватывающих 190 кв. мм. кожной поверхности с тринадцати различных мест тела. Для окраски ядер был взят гематоксилин Delafield'a, а в качестве противоположной ему краски—эозин (в некоторых случаях—кислый фуксин).

Ткань, взятая с подошвы ноги, содержит мейснеровы тельца, но в тканях, взятых с мест, покрытых волосами, не были найдены специализированные окончания подобного рода. Обзор ряда других работ не обнаруживает расхождений с указанными данными, за исключением работы Фрея (Frey), нашедшего единственное тельце Догеля в коже руки.

Оригинальная работа, содержащая значительно больше подробностей, чем настоящая статья, будет опубликована в трудах психологических лабораторий Кларкского Университета.

**Джильберт (Gilbert).**

# THE FUNCTIONAL RÔLE OF FINGER TOUCH AND DAMPER-PEDALLING IN THE APPRECIATION OF PIANOFORTE MUSIC*[1]

*From the Psychological Laboratories of The Johns Hopkins University*

CHRISTIAN PAUL HEINLEIN

In the appreciation of pianoforte music, critical judgments are often passed by the listeners concerning the so-called "finger touch" of the performer. If you should make an inquiry into the opinions held by any large audience of appreciative listeners familiar in a general way with piano music and technique, you would probably be surprised to learn that the majority entertain the singular notion that a performer's interpretation of a given composition is related in some strange manner to a mystical spirituality inherent in keyboard touch. You will hear it said that a certain rendition is beautiful because the performer possesses "a marvellous, mysterious touch." In some few cases, the phrase "piano touch" is employed interchangeably for facility in keyboard technique; however, such judgments are often conveyed with the intention of ascribing to keyboard touch something quite mystical, something spiritual, something inexplicable and unanalyzable, something, as it were, transcending the fundamental characteristics common to all sentienda. Auditors, emotionally enthralled by the vague, transient charm of music yet totally ignorant of the few physical principles which underlie tonal expression, have been heard to exclaim that a personal, magnetic force is often conveyed from the fingers of the artist to the keys of the piano.

Strike a piano key, and what do you have? A resultant tone with position in space and time, subject to change in extensity (1, 7), intensity, protensity, and quality. Only this and nothing more. Piano touch per se is not a separate attribute of the tonal complex, and it does not in itself impart one single iota more to the entire scheme. There is no sensorial condition which transcends dependence upon these fundamental characteristics common to

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[1]The investigation recorded in this paper was carried on during the tenure of a National Research Fellowship in the Biological Sciences.

all sentienda. This is a harsh dictum, indeed, to many of those music-lovers who orthodoxically adhere to the belief that artistic pianoforte performance is bathed in many irreducible and unfathomable mysteries. This dictum is unpopular and unacceptable to the music-lover because it is bounded by some simple laws of mechanics, and any reference to mechanics is taboo in an art which is held to be essentially human (4).

Unfortunately, so many listeners direct their attention to finger, hand, wrist, and arm movements, as if in these movements lay the whole secret of individual interpretation. No one will deny for a single moment that many types of bodily movement function in the formation and development of the performer's various emotional attitudes, and that these same movements may to a large degree impress, react upon, and bias the general receptivity of the auditors. Considered from this angle, it is of no wonder that one artist will produce a different effect upon an audience than will another, or that a baton-wielder of great flexibility will enthuse in an entirely different manner than another conductor of less muscular motility (8, chap. 2 and 3). However, one must remember that, although possessing an important value in themselves, these conditions are extrinsic to the tonal complex. In the case of piano performance, if these extrinsic conditions are eliminated, a tone related in a specific manner to the characteristics limiting it is heard the same whether the sound is produced by one type or by another type of finger touch, wrist and arm movement, or by the mere mechanical stroke initiated by a wind bellows (6).

Facility in finger technique, the acquisition of accurate keyboard control, the ability to coordinate complex moving finger patterns, agility in the production of intensity shadings through appropriate changes in amplitude of stroke, motor synchronization with devious rhythmic patterns anticipatorially perceived—these and many other like features condition any performer's interpretation. The importance of these several methods of control leading to various types of interpretation must not be minimized. Nevertheless, it is quite possible to keep these and many other keyboard conditions, which I have not taken time to enumerate, constant throughout, and yet to present a number of different interpretations, effected by a means usually overlooked, that have to the listener little or nothing in common. In two successive renditions of a given composition, a performer may finger the melodic and harmonic progressions involved

in precisely the same manner, he may in each instance strike definite keys with the same intensity (assuming for the sake of argument a resultant identity of tonal amplitude), he may prolong in each case specific series for an exact temporal extensity, and despite such constancy of conditions he may supply his listener with two very different interpretations. The difference thus effected is not the result of any modification of direct keyboard control (despite the fact that most auditors would unreflectingly pronounce the change a finger-touch modification), but the result of a use of entirely *different damper-pedal patterns.*

The fact that damper-pedal differences are commonly perceived as differences in finger touch is quite convincingly demonstrated in the results obtained from a very simple experiment involving the judgments of 16 selected male college students, eight musically trained and eight musically untrained. These students, all members of a class in general psychology at The Johns Hopkins University, attended the test periods both individually and in small groups of three or four. Provided with comfortable chairs, the test subjects were seated facing a concert-grand model of the Duo-Art reproducing piano,[2] which was located at a hearing distance of approximately 20 feet.

Before experimentation was begun, the subjects were required to read carefully the following sheet of instructions:

> You will hear two renditions of a single musical composition played for you on the concert-grand Duo-Art reproducing piano. You are requested to listen attentively to each performance for the purpose of rendering comparative judgments.
>
> In comparing the two renditions, you are asked to ascertain whether the presentations are identical or different. If you decide that the presentations are different, attempt to ascertain the nature of the differences which you have noted. At the conclusion of the second composition, you will be asked to make a brief note of your judgment in writing.

The music chosen for presentation was Victor Herbert's composition entitled "Kiss Me Again," recorded by Robert Armbruster and played on the Duo-Art reproducing piano (see and hear Duo-Art reproducing roll, no. 67168, Meditations, no. 1). This com-

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[2]The writer wishes to express his deep appreciation and thanks to the Aeolian Company of New York City for their kind cooperation and generous loan of the Duo-Art reproducing piano which made possible this experimentation.

position was chosen for its strong melodic features, its relative simplicity of form, its wide familiarity, and its universality of appeal among the student body. A less familiar piece of any greater form complexity would demand on the part of a listener more concentrated attention to secondary features, make recall more difficult, and in turn vitiate against any simple comparison in judgment (3).

In the first rendition, the composition was played at normal tempo and intensity according to roll indications. In the second rendition, which immediately followed the first rendition after re-roll, the tempo and intensity characteristics were maintained constant with the first rendition, while tonal duration was altered through complete elimination of the damper-pedal action. Tonal duration in the second rendition, therefore, was dependent upon the actual length of note perforation on the roll. The effect produced by this simple change in technique is very striking indeed. Minus pedal action, the melodic contour is fairly well sustained, while the root of the bass which supports the accompanying harmonic figures is frequently played staccato. Elimination of the damper-pedal causes the well-rounded arpeggios to sound sharp and brittle (7, p. 250), while the harmonic background suffers a flippant touch of the jazz in its bare rhythmic beat. Absence of the damper-pedal effects means absence of tonal flux. Pitch points become sharply outlined and often offend through their transiency and evasiveness. The composition is no longer bathed in the delightful richness of soft mellow timbres produced by subtly delicate overtones. The presentation assumes a harshness of unconventionality which neither pleases nor offends directly. Nevertheless, a single simple comparison is sufficient to bear upon and impress the most naïve listener that without damper-pedal effects something essential to artistic interpretation and expression is sorely lacking.

At the conclusion of the second rendition, each subject was given a second sheet on which were printed the following questions:

1) Were the two successive renditions of the composition played exactly the same? Answer yes or no.

2) If you think that the two renditions differed from each other, state the nature of the differences noted.

After the subject had completed his answers to these questions, he was given a third sheet which presented the following form:

The following list includes musical factors which might have been varied in the two renditions which you heard. Place a check-mark beside those

factors which you consider were varied. If there were other factors varied than those listed below, indicate in writing what these were.

The variations which I noted were variations in:

1) Intensity of tone
2) Pitch
3) Touch
4) Time
5) Phrasing
6) Tonal duration
7) Rhythm
8) Accent
9) Dynamics
10) Timbre

Of the above list of factors which you have checked or further noted, which particular factor do you consider was varied the most frequently?

The subject was not allowed to turn this sheet until he had completed his answers. Upon completion, he was requested to answer the questions which were listed on the reverse side. These final questions follow below:

1) Was pedalling employed in either direction?
2) If so—
   What was the nature of the pedalling?
   Was the pedalling the same in both compositions?
3) If you have decided that pedaling was employed, and that the pedalling for the two renditions was not the same, in which rendition was pedalling used the most frequently?
4) What type of pedalling was this?

Of the 16 subjects tested, each subject recognized marked differences between the two renditions. Every subject was familiar with the composition, and preferred the first rendition to the second. A few characteristic replies to the second question of the second sheet follow:

"The first rendition was played with a heavy legato touch which gave the piece an expression of deep feeling. The second rendition was played with a distinctly staccato touch which tended to destroy feeling."

"They differed in the touch of playing."

"The first rendition was easy flowing and pleasing as well as soothing to the ear, while the second was in a short jerky movement not soothing to the ear."

Fifteen of the 16 subjects declared that variation in finger touch produced the chief differences in interpretation. Other factors which

were considered varied in order of frequency were: tonal duration, 14; time, 10; rhythm, 10; phrasing, 9; accent, 9; intensity of tone, 8. No subject mentioned pedalling until this factor was drawn to his attention in question form on the final information sheet. It is also interesting to note that the fact of pedal change produced a number of illusions in judgment. The rendition without pedal was judged to be more rapid in tempo, more accentuated with greater intensity of tone, and more akin to fox-trot rhythm than to waltz rhythm (4, p. 86). One subject remarked that the second rendition was "jazzy." This leads one to believe that tonal duration of an appreciable length tends to vitiate against the musical complex recognized as "jazz."

In recapitulating, it is quite apparent from this brief study that both musically trained and untrained subjects tend to perceive damper-pedal differences primarily as differences in finger touch. In the minds of many, human finger touch still remains the only goddess of musical expression in pianoforte performance. The layman's denunciation that reproducing instruments are a bane to musical art because they lack the feeling power which human touch can render is not justified in the light of some simple laws of mechanics. Human finger touch is subject to the laws of mechanics like any other fact of nature, and the general notion that the artist is capable of transferring a personal magnetism to the keys is simply a notion that is engendered from ignorance of the facts. The musician, and especially the composer, in his feeling attitudes is ofttimes a mystic, and it is just the mystic who shuns any mechanistic conception of art despite the fact that he continually employs mechanical principles to serve his end. A momentary cross-section of any system of aesthetics is based upon a science of some form, and any science of form involves mechanical features. As Whipple (9) and Farnsworth (2) have shown, the reproducing instrument is capable of reenacting to a fine degree of precision the tonal fruits which the artist's finger touch has created in conformity with the mechanical principles of leverage action. Human finger touch has been accorded such a paramount position by the vast majority of listeners that any change in interpretation is likely to be attributed to change of this factor. The important rôle which the damper-pedal plays in expression is little recognized by the layman; yet the most flawless and perfect finger technique, unless supported by adequate pedalling (5), fails to achieve its purpose.

## References


1. Dunlap, K. Extensity and pitch. *Psychol. Rev.*, 1905, **12**, 287-292.
2. Farnsworth, P. R. Comments on the Duo-Art as a laboratory instrument. *J. Appl. Psychol.*, 1928, **12**, 214-216.
3. Heinlein, C. P. A brief discussion of the nature and function of melodic configuration in tonal memory. *J. Genet. Psychol.*, 1928, **35**, 45-61.
4. Howes, F. The borderland of music and psychology. London: Kegan Paul, 1926.
5. Lindo, A. H. Pedalling in piano-forte music. London: Kegan Paul, 1922.
6. Ortmann, O. The physical basis of piano touch and tone. New York: Dutton, 1925.
7. ————. The sensorial basis of music appreciation. *J. Comp. Psychol.*, 1922, **2**, 227-254.
8. Schoen, M. The effects of music. New York: Harcourt, Brace, 1927. Chapter 2 by Myers, C. S. Individual differences in listening to music. Chapter 3 by Ortmann, O. Types of listeners: genetic considerations.
9. Whipple, G. M. A new method of analyzing musical style by means of the reproducing piano. *J. Appl. Psychol.*, 1928, **12**, 200-213.


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## LE RÔLE FONCTIONNEL DU DOIGTER ET DE L'USAGE DE LA PÉDALE FORTE DANS L'APPRÉCIATION DE LA MUSIQUE DU PIANO

(Résumé)

Le fait que les différences dans l'usage de la pédale forte du piano sont ordinairement aperçues comme différences dans le doigter est montré par les résultats obtenus d'une étude où il s'agit du jugement de 8 étudiants universitaires mâles connaissant la musique et 8 qui ne la connaissaient pas. On a demandé à ces sujets de comparer et de rapporter la nature de toutes les différences notées quand on a joué deux fois successivement la composition de Victor Herbert intitulée "Donne-moi encore un baiser" au moyen d'un piano méchanique Duo-Art. On l'a jouée d'abord comme indiquée. Ensuite on a éliminé les effets de la pédale forte tandis que tous les autres effets ont été les mêmes.

Quinze des 16 sujets ont déclaré que la variation du *doigter* a produit les grandes différences d'interprétation, bien que l'on n'eût pas changé le méchanisme du piano contrôlant le doigter. Aucun sujet n'a parlé de la pédale jusqu'à ce qu'on la lui ait fait remarquer. Ils ont considéré d'autres facteurs comme causes de variation: en ordre de fréquence, la durée tonale, le temps, le rythme, la phrase, l'accent, et l'intensité du ton. Bien que le temps, le rythme, et l'intensité du coup des marteaux aient été constants, ils ont pensé que le morceau joué sans pédale était d'un tempo plus rapide, plus accentué avec une plus grande intensité de ton, et plus semblable au rythme du fox-trot qu'à celui de la valse.

Heinlein

## DIE FUNKTIONELLE ROLLE DES ANSCHLAGS UND DES PEDALS IN DER WÜRDIGUNG VON KLAVIERMUSIK

(Referat)

Dass Unterschiede im Gebrauch des Pedals beim Klavierspiel gewöhnlich als Unterschiede des Anschlags angesehen werden, erhellt aus den Ergebnissen einer Untersuchung die mit 8 musikalisch ausgebildeten Studenten und 8 Studenten ohne musikalische Bildung ausgeführt wurde. Den Versuchspersonen wurde Victor Herberts Komposition "Küsse mich noch einmal" zweimal hintereinander von einem Duo-Art Klavier vorgespielt, und beim zweiten Mal alle Pedalwirkungen ausgeschaltet. Nun musten die Vpn. Vergleiche anstellen und die Unterschiede zwischen den beiden Vorträgen sorgfältig notieren.

Fünfzehn der 16 Vpn. erklärten, dass Variation im Anschlag die Haupttunterschiede in der Interpretation verursacht hätte, obwohl der Mechanismus der den Anschlag kontrollirt, nicht verändert worden war. Keine der Vpn. erwähnte das Pedal, bis man sie schliesslich darauf aufmerksam machte. Andere Faktoren die erwähnt wurden sind in der Reihenfolge der Häufigkeit, Tonlänge, Takt, Rhythmus, melodische Gruppirung, Nachdruck und Tonstärke. Trotzdem dass Tempo, Rhythmus und Tonstärke unverändert blieben, so wähnte man doch bei dem Vortrag ohne Pedal ein schnelleres Tempo, mehr Nachdruck und grössere Tonstärke wahrzunehmen und meinte das Klavierstück sei dem Fox-Trot näher verwandt als dem Walzerrhythmus.

HEINLEIN

## ФУНКЦИОНАЛЬНАЯ РОЛЬ ПАЛЬЦЕВОГО УДАРА И УПОТРЕБЛЕНИЯ ЗАГЛУШАЮЩЕЙ ПЕДАЛИ В ФОРТЕПЬЯННОМ ИСПОЛНЕНИИ.

(Реферат).

При опросе 8 музыкально-образованных и 8 музыкально-необразованных студентов колледжа оказалось, что изменения в фортепьянном звуке, происходящие от употребления заглушающей педали, обычно воспринимаются как изменения в качестве пальцевого удара. Испытуемые должны были сравнить и охарактеризовать все изменения отмеченные или при двух последовательных исполнениях пьесы Виктора Герберта „Kiss me Again", сыгранной на репродуцирующем рояле „Duo-Art". Первое исполнение следовало всем обычным правилам. При втором все детали исполнения остались те же, кроме левой педали, которая была уничтожена.

Пятнадцать из 16 испытуемых нашли, что главное различие в исполнении было вызвано изменениями пальцевого удара, несмотря на то, что механизм, контролирующий особенности удара, не был включен. Ни один из испытуемых не подумал о педали, пока их внимание не направили в эту сторону. Некоторые указывали на перемену темпа, звучания, ритма, фразировки, акцента и силы руки. Несмотря на то, что темп, ритм и интенсивность удара оставались неизмененными, испытуемые указывали, что исполнение без педали быстрей по темпу, более акцентировано, более интенсивно по звуку и по ритму ближе к фокстроту, чем к вальсу.

**Гайнлайн (Heinlein).**

# AREA, COLOR, AND BRIGHTNESS DIFFERENCE IN A REVERSIBLE CONFIGURATION*

*From the Psychological Laboratories of Clark University*

CLARENCE H. GRAHAM

## I

The literature on "figure and ground" is quite definite in its view that area is a very important factor in the perception of the phenomenon. Wever (4, p. 55) writes, in describing configurations which elicit most equivocal responses: "One of the factors in equivocality is equality of the areas of the two stimulus fields." Rubin (3) also recognizes this factor when he writes: "One can advance the following, fundamental law: If of two homogeneous, differently colored fields the one is noticeably larger than the other and surrounds the other, then there is an overwhelming probability that the small, inclosed field is perceived as figure."[1] Here he accepts color difference as a factor also.

This study is an attempt to get some quantitative measure of the influences of both area and color in a reversible configuration. The method is one of varying these factors in such a figure, measuring reversals, and analyzing the results. It is hoped that such a procedure will give a more definite, quantitative measure of the part that these factors play in configurations.

## II

The figures used as stimuli in this experiment were similar to the drawing in Figure 1. This is a figure with which Köhler has worked, and is a modification of some of the figures used by Rubin, in that there are six sectors instead of eight. In certain ways this figure is better suited to the purpose than Rubin's. Since there are

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[1]"Man kann folgende fundamentale Regel aufstellen: Wenn von zwei homogene, verschiedenfarbigen Feldern das eine wesentlich grösser als das andere ist und das andere umschliesst, dann ist eine überwiegende Wahrscheinlichkeit dafür vorhanden, dass das kleine umschlossene Feld als Figur aufgefasst wird" (3, p. 79).

only six angles, the range of modification is from 0° to 120°. Rubin's range was 0° to 90°.

The figure was pasted in the center of a neutral gray card, 75 cm. by 55 cm., and consisted of a circle of Hering white, 15 cm. in diameter, divided into sectors by black lines drawn with India ink.

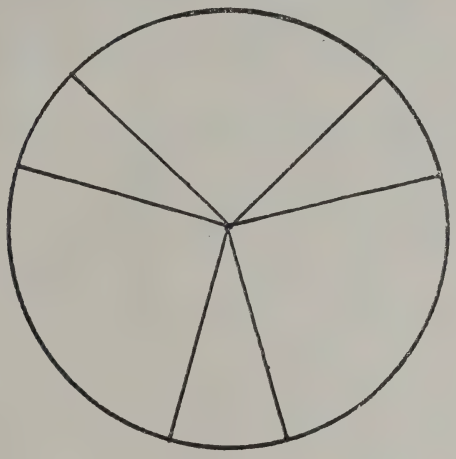

FIGURE 1
A 30° FIGURE

In rare cases the smaller sectors are not seen as "figure." The data of this experiment are selected in the sense that, if an observer reported eye-strain, too great confusion from after-images, etc., after a trial, that trial was thrown out.

In the colored series the sectors alternated, e.g., red, blue, red, blue, etc. The card with the figure on it was hung on the wall 230 cm. distant and on a level with *O*'s eyes. A 200-watt daylight bulb supplied the illumination. This was so placed that it was over and slightly in back of *O*'s head. The rays fell full on the figure, the room, of course, being brightly illuminated.

The apparatus for recording reversals consisted of a kymograph with the standard arrangement of telegraph key. Time was recorded by means of an electric clock in circuit with an electric marker. A chin-rest on a stand was provided for *O*. The telegraph key was placed on the stand in such a way as to be most easily manipulated.

The following observers served in these experiments: Dr. J. P. Nafe, under whose direction the problem was done, F. A. Geldard, and Miss D. E. Johannsen, both assistants in the department. These observers are designated respectively as N, G, and J.

Each observer was given at least one distributed hour of practice in the method of recording, so that when the experiment did begin

there was no uncertainty. *O* was instructed as follows: "You are to see this figure as a reversible configuration. Five seconds before the trial the experimenter will say 'All right.' Two seconds before the trial he will say, 'Ready,' and at the word 'Now' fixate the center of the figure (where the lines meet) and keep fixation throughout the entire trial. Press down when the small areas are 'figure' and release when the large are 'figure.'" Care was taken that instructions were thoroughly understood. Observations were with two eyes.

In the black-white series six figures were used. These figures were those in which the angles between the lines enclosing the smaller areas were respectively 10°, 20°, 30°, 40°, 50°, 60°.

In the blue-white series four figures were used. Blue sectors alternated with white sectors, the angles of the blue sectors being 30°, 45°, 60°, 90°. A black line was drawn in India ink along each contour for the purpose of making conditions as nearly like the black-white series as possible, with the exception of the factor of color. (All colors used were Hering.)

Each observation period consisted of at least 16 trials, and all averages are based on at least 16 cases. Usually only one figure was used during a period. Each trial lasted for 30 seconds, and all averages are in terms of time out of 30 seconds that a system of sectors was seen as figure.

## III

In Table 1 the "Small" column shows the number of seconds out of 30 that the small sectors were seen as figure. The time for the large sectors as figure can, of course, be found by subtracting the time of the small from 30. The "Time" column designates the average time in seconds of each reversal measured from the appearance of Small as figure to the disappearance of Large as figure. "Per cent" column expresses the approximate percentage of total time that Small was figure.

A glance at Table 1 is sufficient to settle the question of the influence of area in reversals, for the black-white series at least. It is immediately seen that, as the area of the small system sectors decreases, the tendency is for them to be more stable as figure. Sectors with an angle of 10° dominate in reversals approximately one and one-half times as long as sectors which are equal, and the areas between vary in dominance (measured objectively in terms of time) in a liminal manner.

TABLE 1

RESULTS FOR THE BLACK-WHITE FIGURE

| Observer | Angle | Small | A.D. | Time | Per cent |
|---|---|---|---|---|---|
| N | 10° | 21.4 | 1.1 | 5.4 | 71 |
| | 20° | 20.6 | 1.2 | 4.6 | 69 |
| | 30° | 19.8 | 0.9 | 4.4 | 66 |
| | 40° | 17.6 | 1.0 | 4.4 | 59 |
| | 50° | 16.7 | 1.3 | 4.0 | 56 |
| | 60° | 15.1 | 0.6 | 3.5 | 50 |
| G | 10° | 23.1 | 0.9 | 3.6 | 77 |
| | 20° | 21.9 | 0.8 | 3.5 | 73 |
| | 30° | 19.6 | 1.0 | 3.3 | 65 |
| | 40° | 18.3 | 1.0 | 4.3 | 61 |
| | 50° | 17.7 | 0.8 | 2.5 | 59 |
| | 60° | 15.0 | — | 2.1 | 50 |
| J | 10° | 22.0 | 0.8 | 3.3 | 73 |
| | 20° | 18.2 | 1.4 | 3.7 | 61 |
| | 30° | 17.6 | 1.5 | 4.1 | 59 |
| | 40° | 16.2 | 0.8 | 5.2 | 54 |
| | 50° | 15.7 | 1.2 | 7.5 | 52 |
| | 60° | 15.1 | 0.8 | 7.0 | 50 |

Is there a rhythm for reversals? We can answer in the affirmative, but with reservations. The most apparent rhythms occurred for N and G at angles of 60°. The comparatively small average deviations for both observers give evidence of this. G reported a perfect rhythm. He reported that the sectors "came in and went out like the beat of a metronome." He further insisted that he was careful not to let a rhythm creep in. N also found a rhythm, although it was not so marked as G's. He reported that "he could almost feel the pulsing of the figure," and for long periods his records show this. However, now and then something would seem to break up the rhythm.

In the beginning of the experiment, trials were one minute in length, but so many factors (such as eye-strain) entered into the situation that the trials were cut to 30 seconds. A continual source of difficulty after the first 20 seconds of the trial was what Ferree (1, p. 114 ff.) has called the streaming phenomenon. At moments in the last 10 seconds, part of the lines or even whole lines were blotted out, but in giving reports all of the observers said that regardless of this they were always sure of a change in the figure. Even though the contours were blotted out, the luminosity and areal distinctions usually remained. There could be no question as to when

a change occurred even under these conditions. We draw no conclusions from the phenomenology of the experiment.

## IV

In the experiments with blue the results are more ambiguous. In order to get a clear idea of what happens, it is better to consider each observer separately.

*Observer G.* Table 2 gives a clear idea of G's results. For him blue adds to the dominance of figure, his results showing that the colored sectors dominate always slightly more than do white sectors of the same area. Furthermore, his results are in harmony with his results in the black-white series, showing the same gradations.

*Observer N.* Table 2 gives the results of observer N. It is apparent that there is not much consistency with former results. In the blue-white series he does not show the trend which he showed in the black-white series. The time of reversals is practically constant, and although there may be a sort of order in his results from 45° up to 90°, the results for the 30° angle are entirely inconsistent with his former results. He reported that he was never certain of a reversal, and, in fact, that if there were any reversals they were very indefinite. Mists and hazes were much more bothersome than in the black-white series. However, he was definitely certain that for him white was dominant.

TABLE 2

RESULTS FOR THE BLUE-WHITE FIGURE

| Observer | Angle | Blue | A.D. | Time | Per cent |
|---|---|---|---|---|---|
| N | 30° | 13.4 | 1.2 | 4.1 | 45 |
| | 45° | 16.5 | 1.7 | 4.3 | 55 |
| | 60° | 14.8 | 1.3 | 4.2 | 49 |
| | 90° | 10.8 | 1.3 | 4.0 | 36 |
| G | 30° | 23.5 | 1.2 | 4.2 | 78 |
| | 45° | 21.6 | 0.6 | 3.1 | 72 |
| | 60° | 16.1 | 1.4 | 2.0 | 54 |
| | 90° | 7.0 | 1.0 | 3.2 | 23 |
| J | 30° | 10.6 | 3.2 | 4.0 | 35 |
| | 45° | 14.2 | 1.2 | 5.4 | 47 |
| | 60° | 7.0 | 0.9 | 5.8 | 23 |
| | 90° | 12.4 | 1.3 | 5.7 | 41 |

*Observer J.* Table 2 gives the results for J. J reported that she could not get figure and ground consistently. Sometimes white would stand out, but blue hardly ever. Usually the sectors did not form a figure-ground configuration, but seemed to be blue and white sectors in the same plane. Sometimes the white would stand out, but the blue would appear only as sectors in back of the white, not as ground. J was certain of only one thing, and that was that white was more dominant than blue.

From this part of the experiment we here draw no definite conclusions. The uncertainty of interpretation of N's and J's results makes it difficult to do this. If their results are to be accepted, it would seem that there are great individual differences in figure-ground experience.

## V

A series of three red and blue figures was used with G. The angles of the red sectors were 30°, 45°, and 60°. Table 3 expresses the results obtained. For G it is seen that in this case the 30° and 45° angles are much more dominant than in the black-white figure. It would seem at first glance as though the increase in dominance must be due to color differences. Furthermore, G reported that red was more dominant. The 60° figure is an exception. The results and the report show that blue is about as dominant as red. However, this figure was presented along with the blue-white figures. It is easy to guess that the observations in the blue-white series carried over a set for this configuration.

At this point the method was changed. From this point on, observations were made monocularly, and with an artificial pupil of 2 mm. diameter. This change was made when it was found that by this method the mists and hazes which the *O*'s had been reporting

TABLE 3

RESULTS FOR THE BLUE-RED FIGURE

| Observer | Angle | Red | A.D. | Time | Per cent |
|---|---|---|---|---|---|
| G | 30° | 24.2 | 0.8 | 8.3 | 81 |
| | 45° | 20.2 | 1.5 | 4.8 | 67 |
| | 60° | 14.0 | 0.9 | 2.2 | 47 |
| N | 30° | 21.2 | 0.6 | 6.8 | 71 |
| (with pupil) | 45° | 18.0 | 0.5 | 4.4 | 60 |
| | 60° | 15.8 | 0.6 | 3.5 | 53 |

were eliminated to a large extent. All the *O*'s reported that judgments were much easier when the artificial pupil was used. A few observations showed that there was no marked difference produced in the results obtained by this method when compared with the results obtained by the former method. However, pupil area, as a possible factor, was controlled.

N's results with the red-blue series are also presented in Table 3. His results also show a slight tendency for the red to dominate.

The ambiguous results obtained from G and J in the blue-white series left us without any conclusions as to the influence of color in this figure. Such results would seem to show uncontrolled factors. The type of reports given and the results obtained led the writer to the belief that brightness differences might be a very important factor. The literature quite generally stresses this possibility. This may have been also a factor in the red-blue series.

It was decided to test the effect of brightness difference, using a modification of the red-blue figure. In this case the brightnesses were equated.

The figure was constructed of gelatine filters cut in the shape of the desired angles. The differently colored gelatines were carefully fitted together and placed upon a square plate of ground glass, 31 cm. by 31 cm. The angular gelatines were kept in place by making them much larger in radius than necessary for the figure, and then by fastening the portions outside the area required for the configuration with gummed paper. A square of black velvet paper (the same size as the glass plates), the center of which had been cut out in the form of a circle of the same size as the black-white series (15 cm. in diameter), was placed over the gelatines. Over this was placed a square of ordinary glass of the same size. The piece of apparatus thus made was merely a complex color filter, giving by means of transmitted light the same type of configuration as was obtained with the pigment colors.

The equating of the brightnesses of the red and blue was done by the flicker method. It was hoped that by using a large enough number of red gelatines to one or two thicknesses of blue that the brightnesses could be equated. However, the number of red filters necessary was so large that a piece of ordinary typewriting paper was used also. The two colors were equated when the ratio was seven red gelatines and one piece of typewriting paper to one blue gelatine. (The white paper lessened the saturation of the red to some extent.)

The apparatus of which this complex filter was a part consisted of a large light-tight box, 100 cm. in length, 35 cm. in width, and 35 cm. in height. The inside was painted black. The front end of the box was made with a square opening so that the figure in the glass plates could be placed over it exactly. A 50-watt light in the box supplied the illumination at a distance of 50 cm. behind the figure. Thus, when the *O* looked at the stimulus in a dark room, he saw a translucent screen of exactly the same dimensions and of exactly the same appearance as the figures which he had been observing formerly, the only difference being that the brightnesses of the red and blue were equal. The brightness of the observed field was 16 millilamberts.

Observations were made in a dark room. The *O* was seated at a distance of 160 cm. from the stimulus. All of the other conditions were the same as in previous experiments. Observations were for 20 seconds. Fifteen trials were given to each figure. At this point M. N. Crook (C) was substituted for G, G no longer being a member of the department.

Before the series with equated brightnesses, a few observations were made with J and C with figures of unequal brightness. This was done in order to get a more satisfactory comparison than G's and N's results with pigments would give. Blue in this case was figure as compared with the series with G and N, in which red was figure. In this case the red was made brighter by simply taking out the white paper from behind the red gelatines.

The results for both *O*'s are given in Table 4. It is apparent that with brightnesses unequal the influence of area is greatly affected. In the case of the 30° blue angles (in which opportunity is very favorable for blue to dominate) blue dominates less than half the time, and in the case of the 60° angles, when, according to the conditions of area, the figure should be most equivocal, the red dominates.

TABLE 4

RESULTS FOR THE BLUE-RED FIGURE (RED BRIGHTER—TRANSMITTED LIGHT)

| Observer | Angle | Blue | A.D. | Time | Per cent |
|---|---|---|---|---|---|
| J | 30° | 7.6 | 1.6 | 5.5 | 38 |
| | 60° | 6.2 | 0.4 | 3.4 | 31 |
| C | 30° | 9.6 | 0.9 | 3.4 | 48 |
| | 60° | 8.3 | 1.3 | 3.8 | 42 |

Let us in this connection refer again to Table 3. This gives N's and G's results for the red-blue pigment figures. It was pointed out in discussing their results that there is a slight tendency for red to dominate, a fact which is shown much more clearly by C's and J's results with this figure.

Table 5 gives N's, J's, and C's results when the brightnesses are equated. It is obvious that the results of the three *O*'s are consistent with respect to percentage. J's values are lower than the values for the other two, but when these results are compared with the results in the series of unequal brightnesses a very definite trend is shown, i.e., when "figure" and "ground" are of equal brightness, area is a most important factor in the perception. The smaller one field is in relation to another, the longer it will dominate in reversals. This seems to be the case even when "figure" and "ground" differ in color. When they are of unequal brightness, the influence of area is enhanced or overcome, depending on whether the larger or smaller area is brighter.

There seems to be an exception to this. G's results with the blue-white figure show the same trend as his results for the black-white figure. This is opposed to the majority of cases. We cannot explain it except as a difference in set. Certainly, in this type of experiment, set is a factor which should be controlled rigidly. Passivity, above all else, should be stressed. Figures used should be divorced as far as possible from the past history of the subject. For this reason equivocal figures presenting black and white areas would

TABLE 5

RESULTS FOR THE BLUE-RED FIGURE (BRIGHTNESS EQUATED—TRANSMITTED LIGHT)

| Observer | Angle | Blue | A.D. | Time | Per cent |
|---|---|---|---|---|---|
| N | 30° | 12.1 | 1.6 | 7.0 | 61 |
| (Left eye) | 45° | 11.4 | 1.7 | 7.8 | 57 |
| | 60° | 10.0 | 1.7 | 9.2 | 50 |
| | 90° | 6.6 | 1.4 | 7.2 | 33 |
| C | 30° | 12.3 | 1.2 | 2.9 | 62 |
| (Left eye) | 45° | 10.5 | 0.8 | 2.7 | 53 |
| | 60° | 9.8 | 0.9 | 3.1 | 49 |
| | 90° | 6.1 | 0.7 | 3.1 | 31 |
| J | 30° | 10.4 | 2.2 | 6.4 | 52 |
| (Left eye) | 60° | 8.6 | 1.9 | 5.1 | 43 |

not seem to be good, for the subject's past experience with printed material, etc., would create a set for the black as "figure."

## VII

In order to check further on these results, a red-green figure was used. The provision of a new figure was made in order to see whether the trend shown with the other figures would apply in this case. Two more *O*'s were used, Miss L. M. Harden and N. L. Munn (H and M), for the purpose of recording the responses of two *O*'s who were completely unacquainted with the problem.

The ratio of green gelatines to red was 2 to 1. The brightness of the observed field was 94 millilamberts.

Table 6 gives the results of all five *O*'s for this series. It is seen that the results are all consistent, in so far as ratio of dominance is concerned. The two new *O*'s give results comparable to those of the others. This series merely substantiates the fact brought out in the last series, that when in a configuration, "figure" and "ground" are of equal brightness, relative area is a most important factor.

## VIII

We have not considered as yet the facts concerned with the period of reversal—those notations listed under the column marked "Time."

TABLE 6

RESULTS FOR THE RED-GREEN FIGURE (BRIGHTNESS EQUATED—TRANSMITTED LIGHT)

| Observer | Angle | Red | A.D. | Time | Per cent |
|---|---|---|---|---|---|
| N | 30° | 13.4 | 0.8 | 3.7 | 67 |
| (Left eye) | 45° | 11.8 | 1.1 | 4.9 | 59 |
| | 60° | 10.5 | 1.3 | 8.7 | 53 |
| C | 30° | 12.3 | 0.6 | 2.6 | 62 |
| (Right eye) | 45° | 11.3 | 0.7 | 2.8 | 57 |
| | 60° | 10.1 | 0.4 | 2.4 | 51 |
| J | 30° | 11.6 | 1.4 | 5.4 | 58 |
| (Left eye) | 45° | 10.6 | 1.1 | 3.3 | 53 |
| | 60° | 9.0 | 1.8 | 4.8 | 45 |
| H | 30° | 13.0 | 1.9 | 8.8 | 65 |
| (Left eye) | 45° | 10.6 | 1.2 | 4.0 | 53 |
| | 60° | 9.7 | 1.7 | 4.7 | 49 |
| M | 30° | 12.3 | 1.2 | 8.5 | 62 |
| (Right eye) | 45° | 11.4 | 1.0 | 6.6 | 57 |
| | 60° | 10.2 | 1.1 | 5.0 | 51 |

It was mentioned in commenting on the results with the black-white series that there was a rhythm for G with the 60° angle, and a rather definite rhythm for N for the same figure. These results are the only ones that show such a tendency.

In analyzing such data, one gets pretty close to the work done on the problem of the so-called "fluctuations of attention." Guilford (2), working with weak visual stimuli, found that "fluctuations are most rapid at a midpoint which is near the limen for fluctuations and become less frequent as the stimulus is made more or less intense."

In the figure used here, there can be no variation in the intensity of the figure, as the areas of the sectors are varied. However, there would seem to be something like a liminal gradation in the response as the areas are changed, the ratio of dominance of one system of sectors to the other being one to one when the areas are equal. As the areas are varied with relation to each other, the dominance measured in time varies.

Is there any similar definite trend for the periods of reversal? With N and G in the black-white series, we find such a trend. Fluctuations are more rapid at the 60° angle and become less frequent as the angle gets smaller. J's results are opposed to these. M and H show results with the red-green figure, which are comparable with N's and G's results in the black-white series. In the red-blue and red-green series, N gives results which are opposed to his former results. The greatest time of fluctuation occurs at 60°. J, with both the red-green and blue-red figures, shows a slight tendency for reversals to occur more rapidly at 60°. C's periods of reversals do not vary definitely in any direction.

Such results as these show nothing definite. They can be explained only when all the facts of the so-called "fluctuations of attention" are presented.

Another problem which presents itself is the problem of the influence of color. We have not done very much with this. Certainly the results would seem to show that color is not a very great factor in the determination of the ratio of the dominance of one system of sectors to another. However, there may be some likelihood that color is an important factor in the period of reversal. The question of the relation of the sensory intensity of the stimulus and the phenomenon of reversal has not been touched. These problems might be studied with indicative results.

## Summary

In this problem we studied the relative contributions of area and color (with and without equating for brightness) to dominance of "figure." The figure used was a suitable reversible configuration. Kymograph records of reversals were obtained. The criterion of dominance was the length of time (out of a definite period) one system of fields was seen as "figure" as compared to the length of time the other system of fields was seen as "figure." By varying the areas of the two systems, by introducing color, and by equating the colors for brightness, we obtained a quantitative expression of the influence of area, color, and brightness differences in reversals.

Our results show that in this type of configuration (1) the system of smaller fields is chosen as "figure," and a relative decrease in the size of these fields makes them more dominant, i.e., they persist as "figure" longer in reversals. Dominance varies inversely in a liminal manner with area. When the systems are equal in area, the configuration is most equivocal. (2) When "figure" and "ground" are of equal brightness, area is a most important factor in the perception. This seems to be the case, regardless of color differences. (3) When "figure" and "ground" are not of equal brightness, the influence of area is greatly affected, and, if the brightness differences are great enough, the tendency is for the brighter system to dominate. (4) There is no indicative tendency shown in the length of the period (or frequency) of reversal.

## References

1. Ferree, C. E. The intermittence of minimal visual sensations. *Amer. J. Psychol.*, 1908, **19**, 58-129.
2. Guilford, J. P. 'Fluctuations of attention' with weak visual stimuli. *Amer. J. Psychol.*, 1927, **38**, 534-583.
3. Rubin, E. Visuell wahrgenommene Figuren. Copenhagen: Gyldendalska Boghandel, 1921.
4. Wever, E. G. Attention and clearness in the perception of figure and ground. *Amer. J. Psychol.*, 1928, **40**, 51-74.

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## LA DIFFÉRENCE DE SUPERFICIE, DE COULEUR, ET DE CLARTÉ DANS UNE CONFIGURATION RENVERSABLE

(Résumé)

Dans ce problème il s'agit des contributions relatives de la superficie et de la couleur (avec et sans rendre égale la clarté) à la dominance de "figure." La figure employée a été l'adaptation d'une des figures de Rubin par Köhler. On a obtenu des notations des renversements par le kymographion. Le critérium de dominance a été la durée relative dans une période définie pendant laquelle on a vu un système de champs comme "figure" comparée à la durée relative pendant laquelle on a vu l'autre système comme "figure." Au moyen de varier la superficie des deux systèmes, d'employer la couleur, et de rendre égale la clarté des couleurs, on a obtenu une expression quantitative de l'influence des différences de superficie, de couleur, et de clarté dans les renversements.

Les résultats montrent que dans ce type de configuration (1) le système des plus petits champs est choisi comme "figure," et une décroissance relative de la grandeur de ces champs les rend plus dominants. La dominance varie inversement d'une façon liminale avec la superficie. Quand les systèmes ont la même superficie, la configuration est le plus équivoque. (2) Quand la "figure" et "surface" ont la même clarté, la superficie est un facteur très important de la perception. Cela semble vrai malgré les différences de couleur. (3) Quand la "figure" et la "surface" n'ont pas la même clarté, l'influence de la superficie est beaucoup influencée, et si les différences de clarté sont assez grandes, le système de plus grande clarté tend à dominer. (4) Nulle tendance indicative ne se montre dans la durée de la période (ou fréquence) de renversement.

GRAHAM

## UNTERSCHIEDE DES FLÄCHENINHALTS DER FARBE UND HELLIGKEIT BEI EINER UMKEHRBAREN GESTALT

(Referat)

Bei diesem Problem handelt es sich um die verhältnismässige Bedeutung des Flächeninhalts und der Farbe (mit und ohne Rücksicht auf Helligkeit) bei dem Hauptfaktor, "Gestalt." Die Gestalt war Köhlers Anpassung einer Figur Rubins. Man machte Platten der Umkehrungen mit einem Kymographen. Der Massstab des Hauptfaktors war der verhältnismässige Zeitraum einer bestimmten Periode während dem ein System von Feldern als "Gestalt" gesehen wurde, im Vergleich zu dem verhältnismässigen Zeitraum während dem das andere System als "Gestalt" gesehen wurde. Durch Veränderung des Flächeninhalts der beiden Systeme, durch Einschaltung von Farbe und durch die Gleichung der Helligkeit, erhielt man ein quantitatives Mass des Einflusses der Unterschiede des Flächeninhalts sowie der Farbe und Helligkeit bei Umkehrungen.

Die Ergebnisse zeigen dass bei dieser Art der Umgestaltung (1) Das System kleinerer Felder als "Gestalt" gewählt wird, und die relative Verkleinerung dieser Felder sie stärker hervortreten lässt. Der Hauptfaktor variiert umgekehrt schwellenartig mit dem Flächeninhalt. Wenn die Systeme den gleichen Flächeninhalt haben, so ist die Gestaltung am zweifel-

haftesten. (2) Wenn Gestalt und Hintergrund von gleicher Helligkeit sind, so ist der Flächeninhalt der wichtigste Faktor in der Wahrnehmung. Dies scheint der Fall ganz abgesehen von Farbenunterschieden. (3) Wenn "Gestalt" und "Hintergrund" nicht die gleiche Helligkeit haben, so wird der Einfluss des Flächeninhalts stark afficirt, und wenn die Helligkeitsunterschiede bedeutend genug sind so wird wohl das hellere System vorherrschend wirken. (4) Man fand keine Anzeichen in der Länge der Periode (oder Häufigkeit) der Umkehrung.

GRAHAM

## РАЗЛИЧИЯ В ПОВЕРХНОСТИ, ЦВЕТЕ И ЯРКОСТИ ПРИ НЕУСТОЙЧИВЫХ КОНФИГУРАЦИЯХ.

(Реферат).

Эта проблема рассматривается с точки зрения отношения поверхности и цвета (с приравнением и без приравнения к яркости) к доминированию „фигуры". Употреблялась одна из фигур Рубина, проверенная Келером. Были получены кимографические записи смены конфигураций. Критерием доминантности являлась относительная длина времени определенного периода, когда одна система воспринималась как „фигура", по сравнению с относительной длиной времени, когда за „фигуру" принималась другая система. Варируя поверхности двух систем, вводя цвет и уравнивая цвета в отношении яркости, было найдено количественное выражение влияния различий в поверхности, цвете и яркости на смену конфигураций.

Результаты показали, что при этом типе конфигурации 1) за „фигуру" принимается система меньших поверхностей, и постепенное уменьшение величины этих поверхностей увеличивает их доминантность. Изменение доминантности обратно-пропорционально изменению поверхности. Когда системы равны по поверхности, конфигурация сомнительна. 2) Когда „фигура" и „фон" равны по яркости, то поверхность является важнейшим фактором восприятия. Это не меняется при перемене цвета. 3) Когда „фигура" и „фон" не одинаковы по яркости, то влияние фактора поверхности уменьшается; при достаточно сильной разнице в яркости доминирует наиболее яркая система. 4) В отношении длины периода доминирования конфигурации никакой определенной тенденции не наблюдается.

**Грехэм (Graham).**

# GEOTROPIC ORIENTATION IN ARTHROPODS: IV. THE BEETLE *PYROPHORUS**

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## I

The manner and explanation of geotropic orientation in a variety of animals have been described in detail (3, 4, 5, 6, 9, 8). For insects (caterpillars, beetles), Crozier and Stier (5, 6) have been able, by reference to the mechanics of simple creeping, to formulate mathematical expressions for the respective types of response. This had previously been accomplished for rats (4) and mice (2, 7), but the mechanical differences in modes of support and progression between mammals on the one hand and arthropods on the other involve necessarily different formulae for their expression (1). The essential factor is the differential pull on the leg and body muscles of each side as altered by change of position of the animal's center of gravity with relation to the angle of inclination of the creeping plane, while the animal is in motion (3).

In the case of arthropods, as indeed with rats and mice, it must be assumed that the orientation of the animal during progression, as manifested in its creeping path, is an expression of the sensible equality of impressed muscular tensions on each side of the animal (5, 6).

## II

The elaterid beetle *Pyrophorus noctiluca*[1] is especially well adapted for investigations of this sort, since it is possible to rule out entirely the factor of light. This is of considerable importance, since the amount of light adequate to stimulate is very small. Even in the presence of a red "safe" light, the animals were photopositive.

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[1]We are greatly indebted to Mr. Robert M. Gray, Superintendent of the Harvard Biological Laboratory, Soledad, Cienfuegos, Cuba, for kindly supplying us with these animals.

TABLE 1

MEAN ANGLES OF UPWARD ORIENTATION OF FIVE *Pyrophorus noctiluca* DURING CREEPING UPON A PLANE INCLINED AT VARIOUS ANGLES ($\alpha$) TO THE HORIZONTAL

The values of $\theta$ are each the mean of 25 trails, five on each of five individuals.

| $\alpha$ | $\theta$ | P.E. $\theta$ as per cent mean $\theta$ |
|---|---|---|
| 10° | 27.4°±1.36° | 5.95 |
| 20° | 42.2°±0.79° | 1.87 |
| 25° | 48.0°±0.60° | 1.26 |
| 30° | 54.1°±1.90° | 3.52 |
| 35° | 61.8°±0.86° | 1.39 |
| 40° | 68.6°±0.63° | 0.92 |
| 45° | 75.5°±1.01° | 1.33 |
| 50° | 82.3°±0.56° | 0.56 |

*Tetraopes* (6) is usually geotropically active only in the light. *Pyrophorus* possesses two large luminous organs on the dorsal surface of the head, one near each lateral posterior margin. During creeping these organs are usually luminescent, or, if not, a gentle tap on the elytra will induce it. The light produced is more than sufficient to permit following the orientation path with great ease. A straight edge was placed parallel to the path of creeping while the animal was in motion, and the angle of upward orientation on the plane, $\theta$ (5), measured with a protractor. In this way the entire path of progression was observed for a distance of 40 cm. The animals were started from a horizontal position near the base of the creeping plane. As emphasized in several previous cases, excessive handling invariably causes loss of ability to orient, and gives inconsistent paths and nonsensical results. To avoid all but the necessary modicum of handling, the animals were permitted to creep about for a few minutes on the creeping surface confined under a petri dish, before a trail was begun and recorded.

The animals crept on black velvet spread smoothly over a flat surface. Velvet was found the most favorable surface for use with these animals, since the soft nap allowed proper "gripping" without injury to the tarsal claws, an important point for normal locomotion. The individuals used for tests must be carefully chosen with regard to the normal condition of the legs and feet. The upper limit at which readings could be taken was $\alpha=50°$; above that inclination the animals fell off the creeping plane. Below $\alpha=10°$, readings were not consistent.

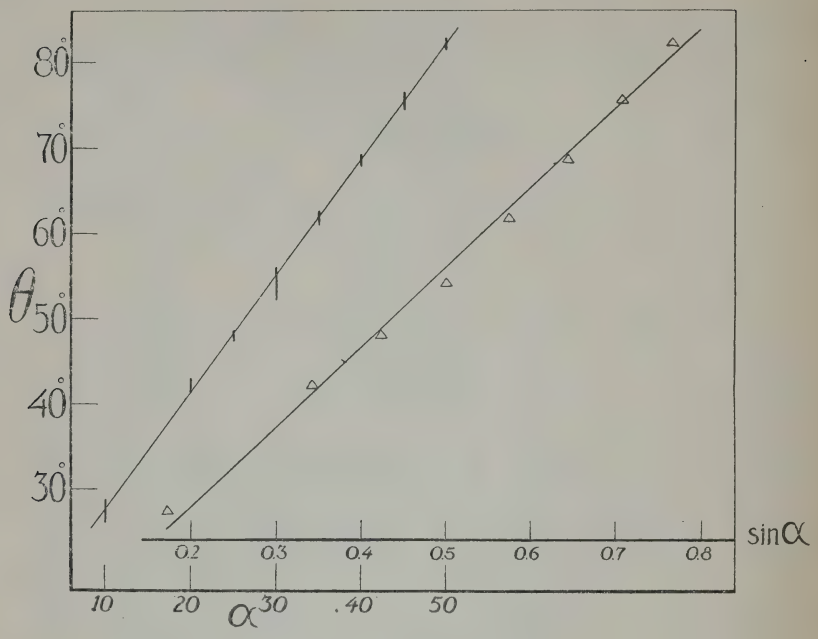


FIGURE 1

MEAN ANGLES OF UPWARD ORIENTATION OF FIVE *Pyrophorus* DURING CREEPING UPON A PLANE INCLINED AT ANGLES $\alpha$=10° TO 50°, INCLUSIVE, TO THE HORIZONTAL

The center of each plotted bar indicates the mean of twenty-five trails; the height is equal to 2 P.E. Plotted triangles indicate the direct proportionality to the angle of orientation $\theta$ with sin $\alpha$

## III

As with the beetle *Tetraopes* (6), and the fiddler crab *Uca* (8)—although for different reasons—the average angle of orientation $\theta$ is directly proportional to the sine of the angle $\alpha$, $\theta = K \sin \alpha + C$. This relationship is shown in Figure 1. All the points fall on a straight line within the limits of the probable errors of the means. Table 1 and Figure 1 give the results in summary. Within the short range of values of $\alpha$ available in the present case, $\theta$ is linearly proportional to $\alpha$ more closely than to sin $\alpha$. The uniformity of results is so apparent that there can be no doubt of the homogeneity

of the data and of the absence of gross defects in procedure.[2] As in the case of other arthropods, the probable errors of $\theta$ decline with sin $\alpha$ (Table 1).

## Summary

The geotropic orientation of the luminous tropical beetle *Pyrophorus* obeys the equation found for *Tetraopes*. The angle of upward, oriented creeping is directly proportional to the sine of the inclination of the surface. The mechanics of geotropic response is the same in these cases, the different slopes of the lines in the two cases indicating only respectively different constants.

## References

1. Crozier, W. J. Tropisms. *J. Gen. Psychol.*, 1928, **1**, 213-238.
2. Crozier, W. J., & Oxnard, T. T. Geotropic orientation of young mice. *J. Gen. Physiol.*, 1927-28, **11**, 141-146.
3. Crozier, W. J., & Pincus, G. The geotropic conduct of young rats. *J. Gen. Physiol.*, 1926-27, **10**, 257-270.
4. ———. Geotropic orientation of young rats. *J. Gen. Physiol.*, 1926-27, **10**, 519-524.
5. Crozier, W. J., & Stier, T. J. B. Geotropic orientation in arthropods: I. *Malocosoma* larvae. *J. Gen. Physiol.*, 1927-28, **11**, 803-821.
6. ———. Geotropic orientation in arthropods: II. *Tetraopes*. *J. Gen. Physiol.*, 1928-29, **12**, 675-693.
7. Keeler, C. E. The geotropic reaction of rodless mice in light and in darkness. *J. Gen. Physiol.*, 1927-28, **11**, 361-368.
8. Kropp, B., & Crozier, W. J. Geotropic orientation in arthropods: III. The fiddler crab *Uca*. *J. Gen. Physiol.*, 1928-29, **12**, 111-222.
9. Wolf, E., & Crozier, W. J. Orientation in compound fields of excitation: photic adaptation in phototropism. *J. Gen. Physiol.*, 1927-28, **11**, 7-24.

[2]It might be objected that the animal is stimulated by its own light. This is undoubtedly not the case, since the eyes are at the anterior part of the head and separated from the luminous organs by the vault of the head. Light from the luminous organs is thrown to either side of the head and downward, but the eyes are effectively shaded by the curved crest. No readings of normal creepings when the luminous organs were inactive were recorded, but careful observation showed satisfactorily that the angle of orientation $\theta$ was the same for a given value of $\alpha$ whether these organs were luminous or not, and hence exerted no photokinetic effect upon orientation. The animal is certainly not stimulated by its own light.

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## L'ORIENTATION GÉOTROPIQUE CHEZ LES ARTHOPODES. IV. LE SCARABÉE PYROPHORE

(Résumé)

L'orientation géotropique du scarabée luminineux des tropiques *Pyrophore* obéit à l'equation trouvée pour *Tetraopes*. L'angle du rampement orienté vers un lieu plus élevé est directement en proportion avec le sinus de l'inclination de la surface. La méchanique de la réponse géotropique est la même dans ces cas, les différentes pentes des lignes dans les deux cas indiquant seulement des constants respectivement différents.

KROPP

## GEOTROPISCHE ORIENTATION DER ARTHROPODEN: IV. DER KÄFER PYROPHORUS

(Referat)

Die geotropische Orientation des tropischen Leuchtkäfers *Pyrophorous* folgt der Gleichung die man für *Tetraopes* fand. Der Winkel des aufwärts gerichteten Kriechens steht in direktem Verhältnis zu dem Sinus Inklination der Fläche. Die Mechanik der geotropischen Reaktion ist in diesen Fällen dieselbe, indem die verschiedenen Inklinationen der Linien in beiden Fällen nur beziehungsweise verschiedene Konstante andeuten.

KROPP

## ГЕОТРОПИЗМ У ЧЛЕНИСТОНОГИХ. IV. ЖУК PYROPHORUS.

(Реферат).

Геотропическая реакция светящегося тропического жука Pyrophorus (из сем. Elateridae — щелкунов) подчиняется уравнению, найденному для Tetraopes. Угол под'ема, взятый по отношению к направлению движения, прямо пропорционален наклону поверхности. Механизм геотропической реакции в одинаковых случаях один и тот же, различия в наклоне линий в двух случаях указывают лишь на соответственные различия констант.

Кропп (Kropp).

# A DISCUSSION OF THE NATURE OF PIANOFORTE DAMPER-PEDALLING TOGETHER WITH AN EXPERIMENTAL STUDY OF SOME INDIVIDUAL DIFFERENCES IN PEDAL PERFORMANCE*[1]

*From the Psychological Laboratories of The Johns Hopkins University*

CHRISTIAN PAUL HEINLEIN

The story is often related how Safonof, the eminent Russian pianist, emphasized to his pupils the ideally refined use of the pianoforte damper-pedal as a vital means of artistic expression. We are told by Scholes (5) in *Crochets* how Safonof would draw the attention of his whole class to Scriabin's pedalling, telling his pupils not to look at Scriabin's hands but at his feet. "Like Sasha's pedalling!" was Safonof's highest compliment to a pupil. But not many teachers of the pianoforte have taken such a vital interest in the science of damper-pedalling.

It may surprise the reader to learn that pianoforte students in many conservatories of supposedly A-1 standing are ordinarily not taught how to use the pedals; they are simply allowed to drift and are expected by their tutors to pick up for themselves this most subtle art. The question immediately arises: Why are teachers of the pianoforte so lax with any explanation of the use of the pedal? The answer is, probably, a simple one; in all likelihood the majority of teachers do not understand what principles they themselves make use of in pedalling. Eighteen college students, who have had more than four years of pianoforte training, were questioned about their acquisition of pedal control, and in every case except one the students were allowed by their tutors to invent their own rules of pedalling.

In many instances, both the professional pianist and the amateur performer pedal on the whole without any forethought to a system of principles or rules. Some students, who have acquired excellent finger technique, are often deficient in their use of the damper-pedal.

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[1]The investigation recorded in this paper was carried on during the tenure of a National Research Fellowship in the Biological Sciences.

Sometimes the pedal is ignored altogether, while at other times, irrespective of the actual harmonic and melodic outline, it is depressed mechanically at the beginning of each measure. Some students pedal vaguely every now and then, when the fingers are not busily occupied with the keyboard, and a little attention can be directed to the feet. Still other students often use the pedal as a means of covering up gross blunders and inadequate finger technique. Most teachers are really stumped by the complexity of the problem, especially since so few books have been written on the subject, and since the amount of space devoted to it in most works on pianoforte playing is so meager. Descriptive studies for the most part have proved grievously inadequate and incomplete, while little attempt has been made to indicate anything beyond a few broad principles of pedalling.

The writer's purpose in this paper does not include any intention of presenting an elaborate discourse on the systematic use of the damper-pedal. No serious attempt will be made to enlighten the misguided pupil in order to help him retrace his steps into a vista of stereotyped rules. Fortunately, musical art is not stereotyped. Musical art would, indeed, suffer most intensely if interpretation were restricted to an exact and mechanical outline. That renowned pianists do interpret a given musical composition differently is a recognized fact, and precious few are the pedagogues who will dare state that a particular interpretation is the *only* permissible interpretation. Spontaneity, a salient emotional characteristic of the musician, is indeed one of the chief charms of interpretation. An attempt will be made herein to justify, within certain reasonably acceptable limits, the function of variability in musical interpretation, and to establish beyond a question of doubt the damper-pedal as an important, indispensable means and device for expressing individuality of taste in the interpretation of a given musical composition (2).

It is not amiss in this connection to call attention to the fact that there exist two schools of pedalling with divergent and antagonistic aims. The first school, far in the minority, insists that a single definite pedal pattern is the one and only correct one to employ in a given musical phrase, while the second school believes that a given musical phrase may be subjected to several different pedal patterns with satisfactory results, provided that these several different patterns do not flauntingly violate the bedrock principles of simple and effective pedal technique. A brief study of the history of pianoforte

interpretation makes acceptance of the views of the first school of pedalling exceedingly difficult.

Yet despite this fact that it is hard to establish fixed and unchanging principles of pedalling, one must not assume for a moment that there are no real technical difficulties in pianoforte pedalling. Any pedal pattern may not be a relevant one for a given passage. The ear-splitting errors in which so many students indulge is sufficient proof of this fact. Although the declaration may be outrageously dogmatic that a certain pedal pattern is the one right and only right one to use in a given musical phrase, it is not, however, improper to refer to a pedal pattern for a given phrase as being either *appropriate* or *inappropriate* as the case may be.

The persistent prevalence of sympathetic intervals in all music played with undamped strings makes clear the function of the damper-pedal. The damper-pedal, if discreetly employed, is a positive aid in surrounding the music with an atmosphere of appropriate harmonics and overtones. While these generated partials are not perceived as segregated, individual tones, they nevertheless serve to enrich the musical outline and to add a colorful background. This richness in color of tonal background is not always desirable. In a sequence of passing notes, for example, any prolonged sustension of the damper-pedal is superfluous and functions to obscure the melodic contour. Then, too, undue depression of the damper-pedal may lead to the unblending dissonance of superposed antagonistic harmonies.

In regard to the adequate use of the damper-pedal, a number of principles can be mentioned to which, however, no strict and rigid adherence can be maintained. It is desirable, for example, to depress the damper-pedal when the music is so naked in outline that it seems to need tonal support and a fullness of volume. It is a safe principle to change the pedal with a marked change in harmony, such as a progression from dominant to tonic or subdominant to tonic, although even this rule is not observed in instances when the triad form is played diatonically or chromatically in rapid succession for the deliberate purpose of obscuring the harmonic background. In instances where the sustension of the harmony more than compensates for the disadvantage of allowing conflicting tones to vibrate concurrently, the damper-pedal may be depressed. The damper-pedal may ofttimes be depressed to advantage when fortissimo effects are desired (although this is not the chief mission of the pedal, as is commonly supposed); when legato effects are desired; when the melodic scheme

coincides with the harmonic background; in broken chords; in arpeggios; in repeated chords; in sustaining the root of a given harmonic, melodic, or rhythmic phrase; in sustaining the root as melody when supported by a treble harmonic accompaniment; in an accompaniment with arpeggio or chordal repetition; and in silent pressure.

It is desirable to release the damper-pedal in passages where passing notes would be likely to function as disturbing factors; in the repetition of arpeggios and chords in the lower pitch registers; in passages where the harmonic atmosphere becomes dense and obscure with conflicting dissonances; when the bass is chromatic in character; when staccato effects are desired; when chords and arpeggios are repeated in the lower pitch registers; in introducing unaccompanied themes where melodic clearness is most desirable; in the case of detached chords; in contrapuntal music and in fugues. Many more such principles might be listed, but additions would have to be tentative only. The list given above constitutes principles which are recognized as fundamental in effective pedal technique.

One might be led to believe that following a survey of such a lengthy list of rules, the use of the damper-pedal becomes clearly defined. However, this is not true, since any musician will point to numerous instances in which violation of these rules is not only possible but highly desirable and proper. The confusion increases manifold, and any simple solution to the whole problem appears more distant than ever when one turns to the masters of the pianoforte for assistance. Indeed, one of the most serious reasons for the neglect to define more accurately where the damper-pedal is to be used is that the greatest virtuosos do not agree among themselves as to where or where not a particular pedal effect should be introduced. For a given musical phrase, one artist will insist upon rhythmic pedalling, another upon melodic pedalling, while a third will resort to harmonic pedalling. Which of these several interpretations can we consider as the "correct" one?

Since the masters of any art are the greatest authorities of that art, the writer has directed his attention to the pianoforte performances of a few world-famous virtuosos for enlightment and assistance in the process of formulating a feasible hypothesis. Duo-Art[2] repro-

[2]The author wishes to express his thanks and appreciation to the Aeolian Company of New York City for their kind cooperation and generous loan of the Duo-Art reproducing piano which made possible this experimentation. He has also received special permission from the Aeolian Company to publish kymographic records of the reproducing rolls included herein. For an evaluation of the Duo-Art as a laboratory instrument, see (1).

ducing piano records of renditions by Ethel Leginska, Leopold Godowsky, Harold Bauer, and Joseph Hofmann were employed in an analysis of the damper-pedal interpretations of some very familiar compositions. Individual pedal patterns were recorded in picture wave form on a continuous smoke-drum kymograph, which was connected through a series of pulleys to the damper mechanism of the recording piano. This method of studying a specific pianoforte function has many advantages over the method of direct roll analysis such as Whipple (6) has employed. The kymogram presents the pedal performance in the form of a wave pattern, which indicates at a glance the actual characteristics of the pedal depression in terms of incidence, protensity, and amplitude.[3] In a statistical analysis of pedal pattern, the incidence and protensity of depression and release are related to the time factor in seconds and to the pitch points of the melodic contour. These latter two functions are recorded on the kymograph by means of electromagnetic pens. The time pen is connected in circuit with a seconds chronometer, while the melody pen is connected in circuit with a special electric recording device attached to the keyboard mechanism of the reproducing piano.[4] A third electromagnetic pen is connected in circuit with a contact key which records measure or phrase duration. The pen which records pedal depression is connected with the kymograph in such a way that depression of the pedal activates the pen downward, while release of the pedal activates the pen upward.

Utilizing the kymographic method of recording pedal performance, a rendition of Schumann's "Traumerei," Op. 15, no. 7, by Ethel Leginska (Duo-Art roll no. 55860) was compared with a rendition of the same composition by Leopold Godowsky (Duo-Art roll no. 5855-3). The tempo of each rendition was regulated in

[3]It should be noted that the reproducing mechanism of the Duo-Art piano does not provide half-depression or half-release. In other words, the many variations and gradations of pedal amplitude which prevail in a human performance are not reproduced by this instrument. The pedal bellows function in whole and not in part. If a half-depression appears on the kymogram, it is due to the fact that the pedal bellows are somewhat sluggish in responding to a very short pedal perforation on the recording roll. This means that many nuances are introduced in a rendition at points where the artist probably had no intention of introducing them. Despite this artificial condition, the Duo-Art piano is quite satisfactory for the purpose of many comparative studies of the frequency and protensity of damper-pedal depression.

[4]For the method of recording the incidence of melodic pitch points on a kymographic drum, see (3).

TABLE 1

A Direct Comparison of the Damper-Pedal Performances of Ethel Leginska and Leopold Godowsky Playing Rubinstein's Arrangement of Schumann's "Traumerei"

In treating the successive interpretations statistically, the composition is divided into eight phrases of four measures each. The time relationships and the pedal frequency have been computed from kymograms recorded by the Duo-Art reproducing piano.

| Rendition by Ethel Leginska | | | Rendition by Leopold Godowsky | | |
|---|---|---|---|---|---|
| Phrase | Time in seconds | Pedal frequency | Phrase | Time in seconds | Pedal frequency |
| 1 | 17.8 | 6 | 1 | 21.6 | 17 |
| 2 | 19.3 | 7 | 2 | 21 | 16 |
| 3 | 17.5 | 6 | 3 | 19.5 | 17 |
| 4 | 20 | 7 | 4 | 21.1 | 19 |
| 5 | 17.2 | 6 | 5 | 15.8 | 18 |
| 6 | 18.8 | 7 | 6 | 20 | 15 |
| 7 | 17.7 | 6 | 7 | 19.6 | 16 |
| 8 | 28.3 | 6 | 8 | 31 | 17 |
| | | 51 Total | | | 135 Total |

conformity with the roll indications. The intensity lever was shifted to Duo-Art "normal" reading.

For convenience of presentation, the results of each rendition are given statistically in Table 1. In this simple statistical treatment, the composition is divided into 8 phrases of 4 measures each. The time allowed to play each phrase is computed in tenths of a second as the smallest time unit from the first pitch point of one phrase to the first pitch point of a subsequent phrase and is derived from the original kymogram through a micrometric study of the time line by means of a powerful reading lens.

Even a casual glance at the figures represented in this table will convince any reader of the vast difference in the pedal performances of these two outstanding artists. Leginska pedals 51 times during the rendition, while Godowsky pedals 135 times, or nearly three times as many depressions. Leginska allows 2 minutes and 36.6 seconds in which to play the composition, while Godowsky allows 2 minutes and 49.6 seconds. For any given phrase, Leginska never employs the same pedal pattern as Godowsky, nor does she ever allow the same time value. Leginska's pedalling is essentially harmonic in structure, while that of Godowsky is primarily melodic in structure. Leginska's tendency is to shift when there is a basic

change of harmony, while Godowsky's tendency is to pedal passing notes either in the treble or in the bass. Both pianists frequently indulge in syncopated pedalling, i.e., pedalling marked by either anticipation or procrastination and not coincident with the pitch points involved. No attempt is made in this study to relate pedal performance with other musical attributes, such as tonal intensity of either the treble or the bass pitch region and the temporal value of phrase formulation. There is little doubt that these factors, in addition to many other keyboard conditions, do play a major rôle in fashioning the pattern of any pedal performance. However, the author's chief concern at this time is merely to demonstrate the simple, undeniable fact that considerably wide differences do exist between the pedal performances of great artists.

While the figures in Table 1 serve to illustrate some basic differences in performance, the true characteristics of each rendition are revealed only in a study of the pedal patterns as recorded on the

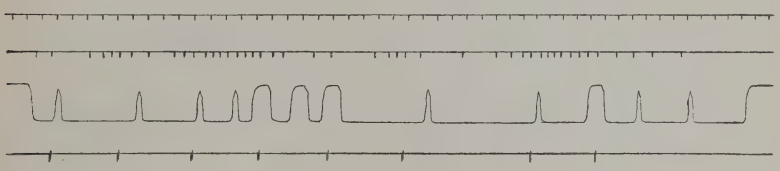

FIGURE 1
LEGINSKA'S RENDITION

original kymograms. Figure 1 is a reduced photostatic reproduction of a kymogram giving Leginska's damper-pedal performance for the last two phrases of Schumann's "Traumerei." The top line is the time line, the line immediately below is the melodic line, which gives the pitch point incidence of the melodic contour; the wave pattern below the melodic line is the pedal line, while the line in lowest position gives the division of musical measures within the phrases.

Figure 2 gives Godowsky's rendition for the same two musical phrases. In a brief comparison of these two renditions, in addition to the wide differences in pedal pattern, the reader will notice some marked differences in the temporal relationships of the pitch points of the two melodic lines. For example, in the last two measures, Leginska, playing somewhat more rapidly, groups her melodic points together in quite a different fashion. While figures are satisfactory

in indicating pedal depression frequency, the protensity characteristics of the pedal pattern are likewise revealed only through a direct analysis of the kymogram. The remark that the damper-pedal was depressed four times within a phrase, although convenient and satisfactory in suggesting the broader aspects of response, does not indicate minute variations in performance, nor does it reveal the actual distribution of the depressions in relation to the melodic contour. Hence, for purposes of illustrating the true variations in pedal per-

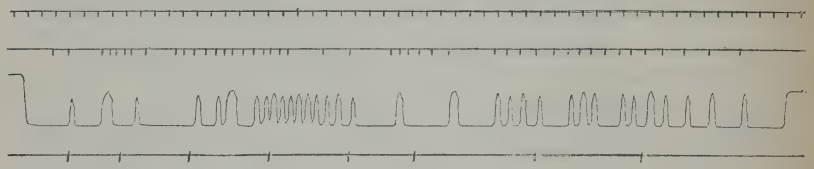

FIGURE 2
GODOWSKY'S RENDITION

formance, Figures 1 and 2, as well as the several subsequent excerpts from the original kymograms, are inserted.

The most naïve listener can readily differentiate between Leginska's performance and that of Godowsky. The performance of Leginska is often bathed in a harmonic background, rich in overtones, whereas the performance of Godowsky, despite greater keyboard sustension, is more sharply outlined and melodically projected from a soft harmonic background unsupported by open strings through frequent release of the pedal. Despite these marked differences in pedal technique, which lead to entirely different musical effects, any thoughtful critic will hesitate to exclaim that either of these renditions is the "correct" one. Since master virtuosos can render such diverse interpretations of a relatively simple composition, and since, after all, musical interpretation is engendered by individual taste and reflects in some measure the emotional characteristics and eccentricities of the performer, any reference to a condition of "correctness" is largely irrelevant. Both of these renditions under consideration are "correct" in so far as neither violates the fundamental principles of pedal decency. From the standpoint of musical merit, either rendition is as "good" as the other.

Yet, admitting the fact that these two entirely different pedal formulations are based upon wholly acceptable technical criteria,

few auditors, indeed, will pass equal judgment on these renditions. From the hedonistic point of view, this condition is to be expected. Levels of appraisal are inevitable from any group of individuals consisting of various perceptual types of listeners. The whole subject of musical appreciation which revolves about this point is so distressingly complex that space limits any further discussion of this aspect.[5]

It is generally held that, because of preciseness of form in phrasal repetition, certain primitive rhythmic patterns are subject to a minimum of variation in interpretation. Such primitive rhythmic patterns prevail in music written in simple march time. Beethoven's "Turkish March," for example, which is a composition characterized by a simple melodic figure superposed upon a simple, conventional harmonic background productive of regular recurring accents, falls within this category. In any reduction of the relatively complex to the relatively simple, one might be easily led to suppose that in musical composition which is characterized by primitive patterns in all attributes, the probability for any marked differences in pedal performance is extremely small. It would seem logical to maintain that pedal differences ought to diminish in scope when a specific formulation of notes tends to delimit the number of interpretations possible. This does not seem to be entirely the case, however.

In comparing Harold Bauer's rendition of Rubinstein's arrangement of Beethoven's "Turkish March" (Duo-Art roll no. 5837-4) with Joseph Hofmann's rendition of the same composition (Duo-Art roll no. 6327-8), one is surprised to learn the marked differences which exist in the pedal performances of these two famous artists. In effecting this comparison statistically, the composition is divided into 24 phrases of four measures each, and four phrases of two measures each. The results obtained from the kymographic method of recording are presented in Table 2. Bauer pedals 99 times during the rendition, while Hofmann pedals 89 times. The pedalling of both pianists is primarily of the rhythmic type, i.e., the pedal is usually depressed on the fundamental measure accent. Bauer allows 3 minutes and 57 seconds in which to play the composition, while Hofmann allows 3 minutes and 51.8 seconds—a remarkably close approximation. In view of the exact rhythmic formulation of the composition, such similarity in time value is to be expected. From the standpoint of distribution, however, no two parallel phrases of

[5]For information regarding the nature of individual differences in listening to music, see (4).

TABLE 2

A Direct Comparison of the Damper-Pedal Performances of Harold Bauer and Joseph Hofmann Playing Beethoven's "Turkish March"

In treating the successive interpretations statistically, the composition is divided into 24 phrases of four measures each, and four phrases of two measures each. The two-measure divisions are indicated by asterisks following the phrase numbers. The time relationships and the pedal frequency have been computed from kymograms recorded by the Duo-Art reproducing piano. The time value of the last phrase of the composition is calculated from the tonal incidence of the initial note of the first measure to the point of final pedal release in the last measure.

| Rendition by Harold Bauer | | | Rendition by Joseph Hofmann | | |
|---|---|---|---|---|---|
| Phrase | Time in seconds | Pedal frequency | Phrase | Time in seconds | Pedal frequency |
| 1 | 9.3 | 4 | 1 | 9.0 | 1 |
| 2 | 9.2 | 2 | 2 | 8.8 | 2 |
| 3 | 9.1 | 5 | 3 | 8.8 | 0 |
| 4 | 8.8 | 3 | 4 | 8.4 | 4 |
| 5 | 9.1 | 3 | 5 | 8.6 | 2 |
| 6 | 8.8 | 4 | 6 | 8.5 | 6 |
| 7 | 8.9 | 4 | 7 | 8.7 | 4 |
| 8 | 9.0 | 4 | 8 | 8.5 | 5 |
| 9* | 4.6 | 2 | 9* | 4.5 | 3 |
| 10 | 9.0 | 4 | 10 | 8.4 | 5 |
| 11* | 4.5 | 2 | 11* | 4.6 | 4 |
| 12 | 9.1 | 3 | 12 | 8.9 | 3 |
| 13 | 9.2 | 3 | 13 | 8.4 | 3 |
| 14 | 9.3 | 4 | 14 | 8.7 | 3 |
| 15 | 9.1 | 4 | 15 | 8.4 | 1 |
| 16 | 9.0 | 4 | 16 | 8.8 | 4 |
| 17* | 4.7 | 2 | 17* | 4.4 | 4 |
| 18 | 9.3 | 4 | 18 | 8.7 | 4 |
| 19* | 5.0 | 2 | 19* | 4.7 | 2 |
| 20 | 9.4 | 4 | 20 | 9.6 | 2 |
| 21 | 9.6 | 4 | 21 | 8.8 | 1 |
| 22 | 9.5 | 4 | 22 | 9.1 | 7 |
| 23 | 9.5 | 4 | 23 | 8.8 | 3 |
| 24 | 9.5 | 4 | 24 | 8.8 | 6 |
| 25 | 9.1 | 3 | 25 | 9.0 | 3 |
| 26 | 9.0 | 4 | 26 | 8.9 | 0 |
| 27 | 8.8 | 6 | 27 | 9.3 | 4 |
| 28 | 7.6 | 3 | 28 | 11.7 | 3 |
| | | 99 Total | | | 89 Total |

either rendition are allowed the same time values. The chief time difference for any two parallel phrases occurs at the close of the composition. Bauer allows 7.6 seconds for the last phrase, while Hofmann allows 11.7 seconds for the same phrase. In regard to a few outstanding differences in pedal performance, in only nine phrases

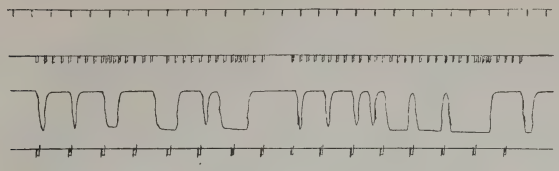

FIGURE 3

HAROLD BAUER'S PEDAL PERFORMANCE OF THE OPENING PHRASES OF THE "TURKISH MARCH"

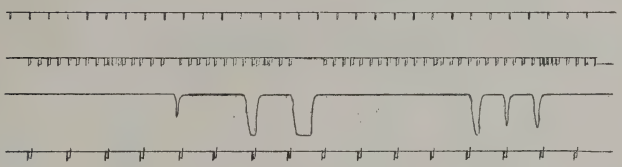

FIGURE 4

JOSEPH HOFMANN'S PEDAL PERFORMANCE OF THE OPENING PHRASES OF THE "TURKISH MARCH"

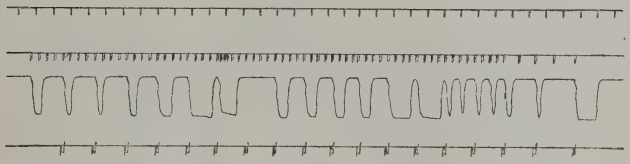

FIGURE 5

HAROLD BAUER'S PEDAL PERFORMANCE OF THE CLOSING PHRASES OF THE "TURKISH MARCH"

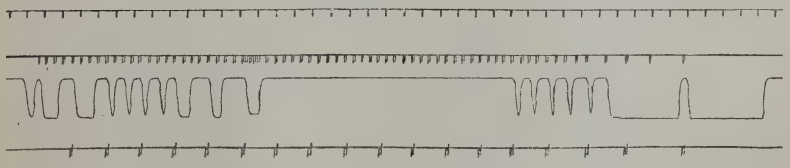

FIGURE 6

JOSEPH HOFMANN'S PEDAL PERFORMANCE OF THE CLOSING PHRASES OF THE "TURKISH MARCH"

TABLE 3

A Comparison of the Damper-Pedal Frequency between a Pianoforte Performance of Schumann's "Traumerei" and its Repetition Following a 10-Minute Rest Interval

The results obtained from four trained musicians are presented in tabular form. The composition is statistically divided into eight phrases of four measures each. An exponent above a number refers to half pedal releases: a sub-numeral refers to half pedal depression. Compare the results below with those given in Table 1.

| Phrase number | First rendition Time in seconds | Pedal frequency | Second rendition Time in seconds | Pedal frequency |
|---|---|---|---|---|
| | | Subject I | | |
| 1 | 18.5 | $10_2$ | 17.2 | $10_3$ |
| 2 | 19.7 | 13 | 19.3 | 12 |
| 3 | 18.3 | $11_2$ | 17.3 | $12_1$ |
| 4 | 19.6 | 12 | 19.4 | 12 |
| 5 | 18.5 | 14 | 18.5 | 13 |
| 6 | 18.2 | 14 | 19.1 | $13_1$ |
| 7 | 17 | $11_1$ | 17 | $8_5$ |
| 8 | 32.4 | 15 | 31 | 14 |
| | | $100_5$ Total | | $94_{10}$ Total |
| | | Subject II | | |
| 1 | 19.2 | $11^1$ | 22.2 | $10^1$ |
| 2 | 22.8 | $10^1$ | 26.2 | $10^1$ |
| 3 | 20 | $12^4$ | 23.5 | 7 |
| 4 | 23 | $13^1$ | 26.7 | $8^1$ |
| 5 | 16.4 | $11^4$ | 20 | $7^2$ |
| 6 | 19 | $12^3$ | 23 | 8 |
| 7 | 18.3 | 9 | 21 | $8^1$ |
| 8 | 26.5 | $10^2$ | 30.9 | 9 |
| | | $88^{16}$ Total | | $67^6$ Total |
| | | Subject III | | |
| 1 | 19.8 | 10 | 20 | 9 |
| 2 | 20 | 11 | 21 | 14 |
| 3 | 19.5 | 11 | 19.3 | 11 |
| 4 | 19.2 | 11 | 21.5 | 13 |
| 5 | 18.3 | $12^1$ | 18.4 | 14 |
| 6 | 20.2 | 12 | 20.5 | 11 |
| 7 | 19.8 | 10 | 20.5 | 12 |
| 8 | 28 | 13 | 27.5 | 15 |
| | | $90^1$ Total | | 99 Total |

| | | Subject IV | | |
|---|---|---|---|---|
| 1 | 20.4 | $11_2$ | 19.4 | $10_1$ |
| 2 | 19.1 | 13 | 21 | 13 |
| 3 | 20 | 12 | 19.2 | $14^1$ |
| 4 | 19.3 | 14 | 20.6 | 13 |
| 5 | 17.7 | 15 | 18 | $13_3$ |
| 6 | 19.8 | 13 | 18.5 | 15 |
| 7 | 17.4 | $12_4$ | 18.2 | 11 |
| 8 | 29.5 | 14 | 29.8 | 13 |
| | | $104_3$ Total | | $102^1_4$ Total |

out of a total of 28 phrases does pedal frequency for the two renditions coincide. And even in these few coincidences, the distribution of pedal pattern in relation to the pitch points involved is quite different. In phrase 3, Bauer pedals five times, while Hofmann does not pedal at all. In phrase 15, Bauer pedals four times, while Hofmann pedals once. In phrase 22, Hofmann pedals seven times, while Bauer pedals only four times. And in phrase 26, Bauer pedals four times, while Hofmann does not give any response. Figures 3 to 6 inclusive are photostatic reductions of the original kymograms, which assist in illustrating at a glance some of the marked differences in pedal performance. Figure 3 gives Bauer's pedal performance for the introductory phrases of the "Turkish March," while Figure 4 gives Hofmann's pedal performance for the very same musical section. Figure 5 shows Bauer's performance for the closing phrases of the composition, while Figure 6 gives Hofmann's performance for the same sequence of measures. In these respective excerpts, the top line is the time line in seconds, the line immediately below is the melodic line, the wave pattern is the pedal performance, while the bottom line indicates the measure span.

Four trained pianists were employed in an experiment which was planned to demonstrate the existence of individual differences in pedal performance and also the extent to which a musician is capable of duplicating voluntarily a pedal response to a given musical composition. Again, Schumann's "Traumerei" was chosen for this purpose. The subjects were carefully examined for their musical ability and were selected from a large group of candidates. Subjects 1 and 2 are conservatory graduates and at the time of experimentation were occupied in professional work. Subjects 3 and 4 have both had the equivalent of four years of preparatory training.

Before selection was effected, in addition to the questionnaire method of obtaining information, each subject was required to prove his ability to perform from memory and to read unfamiliar compositions of moderate difficulty at sight. The compositions chosen for this purpose were those written by Nyiregyhazi, namely, "Theme in A minor," "Fantasy in D minor," "Plaintive Sounds," and "Adagio from a Dramatic Sonata in A-flat minor." These compositions were chosen especially for their unusual modulations and frequency of accidentals introduced through change of key.

As a preliminary task in experimentation, each subject was required to play through "Traumerei" in order to convince the director that he could perform this composition without any significant error. Upon satisfying this requirement, the subject was requested to play the composition a second time, this time, however, observing a definite tempo value. This tempo value was established by presenting to each performer before rendition a series of 16 metronome clicks. An eighth note was evaluated at a metronome indication of 100. In the process of establishing this time value, each subject was instructed to orient himself by a mental count of each metronome beat. Upon cessation of the metronome pendulum at the 16th beat, the performer was then instructed to begin his rendition, and to approximate as a time basis the metronome equivalent. The performer was made to understand, however, that the metronome beat was preliminarily introduced to serve merely as a guide to a desirable tempo, and that his own interpretation was not to be mechanized nor delimited by this specific temporal span. Neither in the preliminary nor in the final instructions was any reference made to pedalling. The subject was simply told to give an artistic rendition of the composition according to his own method of interpretation. No pedal symbols were included in the score from which the subject read. By means of special apparatus concealed from view, the subject automatically recorded his own pedal performance.

Figure 7 is a diagrammatic sketch of the method of recording the pedal performance of the several musicians tested. The pedal rod *R*, which is supported by the pedal leg *L*, activates the damper mechanism within the piano through the depression of the pedal *A*. This pedal rod is connected in series with an inverted N-pulley system, which is capable of transferring to a kymographic drum the amplitude and protensity characteristics of any series of pedal depressions. The pedal pattern is amplified by means of the intermedi-

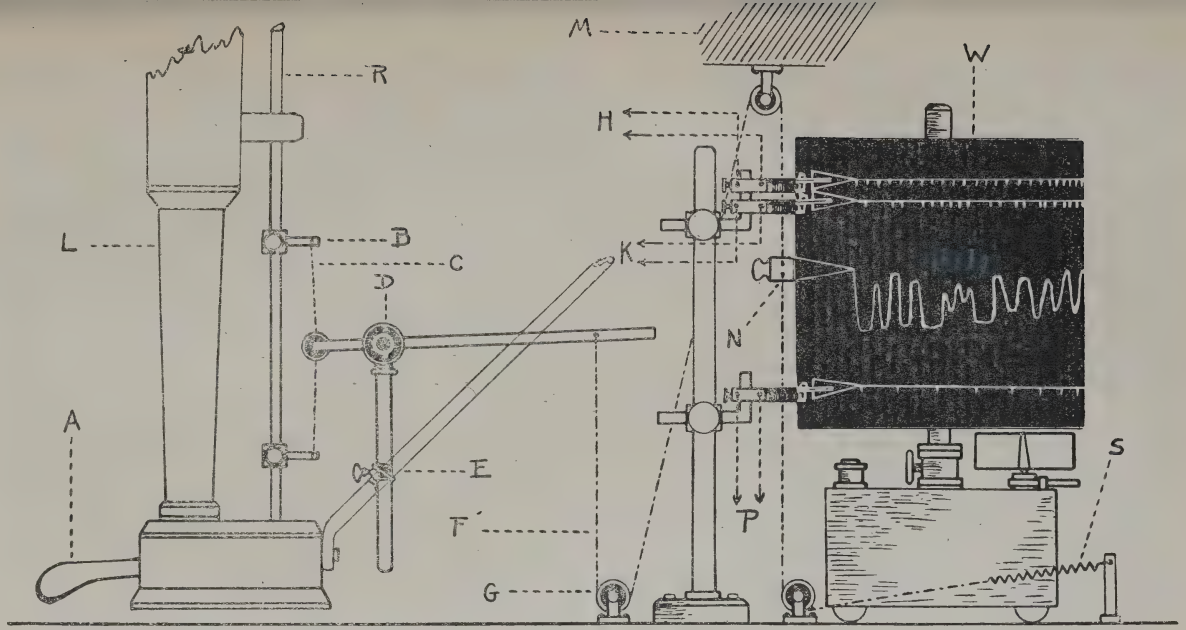


FIGURE 7

MECHANISM FOR RECORDING PIANO PEDAL PERFORMANCE

*A*—damper pedal; *B*—clamp to which is attached heavy silk thread *C*; *D*—fulcrum adjustment of lever device; *E*—clamp attachment to piano support; *F*—heavy silk thread passing through a series of three minute brass pulleys and drawn tense by the spring *S*; *G*—typical brass pulley; *H*—circuit line from seconds pendulum to first electromagnetic pen; *K*—circuit line from melody key to second electromagnetic pen; *P*—circuit line from phrase key to third electromagnetic pen; *M*—piano base acting as support for superior pulley; *N*—damper-pedal recording pen constructed of thin aluminum. Alignment of this pen with the drum is maintained by a twisted winding of the thread. *W*—kymograph drum.

ate lever device *D*, whose fulcrum is guaged at an arbitrary mechanical advantage of three points. Following the use of the reversible pulley system, any depression of the pedal equivalent to an *R* extensity will depress the recording pen *N* for an amplitude equivalent to an extensity of 3*R*. Total release of the damper pedal draws the recording pen upward to zero position. Three electromagnetic pens, whose circuit lines are indicated at H, K, and P, are employed to record (1) the time of performance in seconds, (2) the pitch points of the melodic series, and (3) measure of phrase incidence. As in previous instances, these several functions are recorded electrically, and through this recording process it is possible to obtain a complete picture for purposes of statistical analysis.

After the subject had completed his first rendition of the composition (the preliminary rendition is no longer considered in this discussion), he was dismissed from the music room for a period of ten minutes. During this interval his time was chiefly occupied with reading in the departmental library. This dismissal also gave the experimenter ample time and opportunity to effect the necessary kymographic adjustments for a second record. At the conclusion of the rest interval, the subject was recalled and instructed to give an exact duplicate of his first rendition in every detail. On this second occasion, no preliminary metronome value was indicated. The duty of the subject was to recall his previously established tempo as well as all of the other factors of interpretation. During both renditions, the subject was permitted to read from a score without pedal symbols.

The results obtained from this simple experiment are strikingly significant. *No subject was able to duplicate his previous performance.* On the whole, the professional pianists evidenced greater variation in the duplicate response than did the student pianists. From the introspective reports obtained from the subjects immediately following the second rendition, subjects 2, 3, and 4 declared that they were certain of having achieved an identical repetition. Subject 1 stated that he did not believe it humanly possible to duplicate any given musical performance of appreciable length. The results in terms of pedal frequency are presented in Table 3. In reading this table, which is presented for statistical convenience, the reader should not be misled by the identity of pedal frequency in the case of several parallel phrases. An analysis of the original kymograms shows that no subject had achieved an exact repetition of his initial

pedal performance for any given phrase. In every instance, the pattern distribution for any two parallel phrases was different. In regard to the total time allowed to play the composition, no subject was able to duplicate his first rendition. While subject 4 closely approximated in repetition the time value of his initial performance, it must be remembered that his total time summates from non-coincident phrases.

Following the precise beat of the metronome, the progression of notes constituting the entire composition possesses a time value equivalent to approximately 2 minutes and 32 seconds. The time allowed by each of the four subjects for each rendition of the composition is as follows:

| Subject | First rendition | | Second rendition | |
|---|---|---|---|---|
| | Minutes | Seconds | Minutes | Seconds |
| 1 | 2 | 42.2 | 2 | 38.8 |
| 2 | 2 | 45.2 | 3 | 13.5 |
| 3 | 2 | 44.8 | 2 | 48.7 |
| 4 | 2 | 43.2 | 2 | 44.7 |

Subject 2 shows the greatest amount of variation in performance repetition, both in regard to tempo and in regard to pedal depression. From the standpoint of musical merit, however, both renditions proved equally good. In the first rendition, his pedalling assumed the melodic type, whereas in the second rendition, his tendency was to prolong a specific harmonic background. Differences in the time values of phrasing and differences in intensity shadings were instrumental in effecting these pedal changes. In the first rendition, he made a total of 88 depressions, while, in the second rendition, he made a total of only 67 depressions. In the first rendition, he released the pedal halfway 16 times, while, in the second rendition, he made only 6 such movements.

Subject 1 frequently indulged in half-depressions. In the second rendition, he made twice as many half-depressions as he did in the first rendition. It must be noted that the musical effects produced by half-release and by half-depression are quite different from those produced by complete release or complete depression. Half-depression or half-release gives the harmonic background a blanketed and subdued effect. While resonance still functions, it is diminished to a considerable extent. In contrast to both subjects 1 and 2, subject

3 made a single half-release in the first rendition and no half-depression in either rendition.

The pedalling of subject 4 was essentially melodic. This subject was able to approximate more closely than any other subject the time value as well as the pedal frequency and distribution of his initial rendition. It is singular to note, however, that either of his renditions was the least artistic of the group. His performances, on the whole, were quite stilted and admitted of very few shadings in intensity. Although his tempo was slightly in advance of the metronome basis, his phrasing throughout was unusually rigid and mechanized in conception.

## Summary

1) Teachers of the pianoforte frequently avoid teaching pupils the principles of damper-pedalling. In most instances, the pupils are allowed to invent their own rules of pedalling. Such individual inventive schemes often lead to disastrous results in performance.

2) Probably one of the most serious reasons for the neglect to define more accurately where the damper-pedal is to be used is that the greatest virtuosos do not agree among themselves as to where or where not a particular pedal pattern should be introduced.

3) Employing the kymographic method of comparison, it has been shown that leading artists differ considerably in their pedal interpretations of a given musical composition.

4) Relative to intensity shadings, speed of rendition, and types of phrasing effected, it has been shown that several different damper-pedal patterns may be technically acceptable and musically appropriate for a given composition.

5) Four well-trained pianists, in addition to evidencing marked individual differences in interpretation, were unable to duplicate after a ten-minute interval an initial pedal performance.

## References

1. Farnsworth, P. R. Comments on the Duo-Art as a laboratory instrument. *J. Appl. Psychol.*, 1928, **12**, 214-216.
2. Heinlein, C. P. The functional rôle of fingertouch and damper-pedalling in the appreciation of pianoforte music. *J. Gen. Psychol.*, 1929, **2**, 462-469.
3. ———. A new method of studying the rhythmic responses of children. *J. Genet. Psychol.*, 1929, **36**, 205-228.
4. Schoen, M. The effects of music. New York: Harcourt, Brace, 1927. See chaps. 2 and 3.

5. Scholes, P. A. Crotchets. London: John Lane, 1924.
6. Whipple, G. M. A new method of analyzing musical style by means of the reproducing piano. *J. Appl. Psychol.*, 1928, **12**, 200-213.

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## UNE DISCUSSION DE LA NATURE DE L'USAGE DE LA PÉDALE FORTE DU PIANO AVEC UNE ÉTUDE EXPÉRIMENTALE DE QUELQUES DIFFÉRENCES INDIVIDUELLES DANS L'USAGE DE LA PÉDALE

(Résumé)

On a fait une enquête expérimentale dans le but de constater la nature, la fonction, et le degré de variabilité dans l'usage de la pédale forte du piano par des pianistes fameux. Le piano méchanique Duo-Art a reproduit quelques compositions comme jouées par quatre pianistes célèbres pour rendre possible l'analyse de leur interprétation de ces compositions bien connues. On a noté l'usage individuel de la pédale au moyen d'un kymographion continu de papier au moyen d'une connexion directe avec le méchanisme de la pédale et synchronisé avec la notation musicale au moyen d'une machine électrique spéciale. Les résultats montrent de grandes différences dans l'usage de la pédale par les divers artistes.

On a employé quatre pianistes habiles, deux étudiants, et deux pianistes de métier, pour montrer l'existence de différences individuelles dans l'usage de la pédale et aussi le nombre de fois qu'un musicien est capable de répéter volontairement un usage de la pédale dans une composition donnée. Les résultats de cette expérience montrent que nul pianiste n'a pu répéter son premier usage de la pédale. Dans tous les cas on s'est servi de la pédale d'une façon différente pour deux phrases parallèles. Ces différences sont attribuées aux différences dans le tempo des phrases et aux différences dans les nuances de l'intensité. On conclut que par rapport aux nuances de l'intensité, la vitesse de jouer, et les types de phrases infuencées, plusieurs usages différents de la pédale peuvent être techniquement acceptables et musicalement convenables pour une composition musicale donnèe.

Heinlein

## EINE ERÖRTERUNG DER ART DES GEBRAUCHS DES DÄMPFER-PEDALS AM KLAVIER, NEBST EINER EXPERIMENTELLEN STUDIE EINIGER INDIVIDUELLER UNTERSCHIEDE IN DER ANWENDUNG DES PEDALS

(Referat)

Man unternahm diese experimentelle Untersuchung um die Art, die Funktion und den Grad der Variabilität in dem Gebrauch des Pedals berühmter Klavierspieler zu bestimmen. Duo-Art Klavierplatten der Vorträge vier

hervorragender Klavierspieler wurden in der Analyse der Interpretationen bekannter Kompositionen angewandt. Individuelle Muster des Pedal gebrauchs wurden mit einem continuirlichen Papier Kymographen durch direkte Verbindung mit dem Pedalmechanismus des Klaviers aufgenommen und wiederum synchronisiert mit den Noten der Komposition vermittelst einer besondern elektrischen Vorrichtung. Die Ergebnisse erweisen grosse Unterschiede im Pedalgebrauch der verschiedenen Künstler.

Vier geübte Klavierspieler, zwei Studenten, und zwei Künstler von Beruf, sollten die individuellen Unterschiede im Pedalgebrauch demonstrieren und zugleich dartun in wiefern ein Spieler bei einem Vortrag den Pedalgebrauch nach Wilkür genau wiederholen kann. Die Ergebnisse des Experiments bewiesen dass keine Vpn. den ersten Vortrag genau wiederholen konnte. In allen Fällen war das Verteilungsmuster für irgend welche parallelen Tonsätze verschieden. Diese Unterschiede des Pedalgebrauchs werden wohl durch Unterschiede der Zeitdauer, der Tonsätze und der verschiedenen Abtönung der Intensität bedingt. Man kam zu dem Schluss, dass in Beziehung auf die Abtönungen der Intensität, die Schnelligkeit des Spiels, und die Art der melodischen Gruppirung des Tonsatzes, mehrere verschiedene Pedalmuster für eine gewisse Komposition technisch annehmbar, und musikalisch passend sein können.

HEINLEIN

## ПРИРОДА УПОТРЕБЛЕНИЯ ЗАГЛУШАЮЩЕЙ ПЕДАЛИ И ЭКСПЕРИМЕНТАЛЬНОЕ ИЗУЧЕНИЕ НЕКОТОРЫХ ИНДИВИДУАЛЬНЫХ РАЗЛИЧИЙ В УПОТРЕБЛЕНИИ ПЕДАЛИ.

(Реферат).

Было проведено экспериментальное исследование природы, функций и степени изменчивости употребления заглушающей педали у знаменитых пианистов. Были употреблены записи исполнения известных произведений четырьмя знаменитыми пианистами; записи воспроизводили на репродуцирующем пианино Duo-Art. Индивидуальное употребление педали каждого пианиста было записано на непрерывном кимографе, непосредственно соединенном с механизмом левой педали, и синхронизировано с музыкальными обозначениями путем специальной электрической установки. Результаты показали резкие различия в употреблении педали разными музыкантами.

На примере четырех тренированных пианистов (двух студентов и двух профессионалов) было демонстрировано существование индивидуальных различий в употреблении педали, а также было показано, до какой степени музыкант способен произвольно повторить пользование педалью при вторичном воспроизведении данного музыкального отрывка. Оказалось, что ни один из испытуемых не мог в точности воспроизвести свое первоначальное исполнение. В каждом случае распределение педального рисунка было иное. Эти различия можно приписать разнице во временном распределении фразировки и в интенсивности оттенков. Был сделан вывод, что в зависимости от употребленной интенсивности оттенков, быстроты исполнения и типа фразировки для каждого музыкального произведения можно принять несколько разничных образцов употребления левой педали.

**Гайнлайн (Heinlein).**

# SHORT ARTICLES AND NOTES

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## APPARATUS FOR SMOKING KYMOGRAPH DRUM PAPERS

A considerable technical difficulty which most students, and at times some investigators, have is that of smoking "drums" satisfactorily for record purposes. The best surface is one on which the soot is evenly deposited and is dark enough to make the finished record readily legible. With the ordinary arrangements for smoking, however, the soot is often laid on in blotches, and to make the surface look smooth there is the tendency to go over it a number of times, using more and more smoke until there is produced a uniform black surface. That surface, having a relatively thick carbon deposit, does not offer the best condition for many purposes, because the soot tends to collect upon the writing points and the lines become heavier as the experiment proceeds. Moreover, the user of the kymograph, especially the beginner, wastes much paper in his efforts to get a smooth, not too dark, and not too light surface.

The apparatus, of which working drawings are here shown, enables the beginner to smoke drums corresponding in quality to those of the expert; it saves paper, and it gives the student or the investigator much time which is now dissipated in the acquisition and application of a technique for the main business of his experiment.

The essential parts of the apparatus are the paper covered drum which is to be smoked, the burner, and the mobile relations of these to one another. Figure 1 shows the apparatus in front elevation, and Figure 5 shows it in cross-section (almost end elevation). Figures 2, 3, and 4 show details of the mechanism, so that a good mechanic will be able to manufacture a duplicate. The part which is peculiar, in the sense of being unique and making possible its operation, is the endless or double-pitched screw which moves the smoking burner to and fro.

The general mechanism and operation of the apparatus is as follows (refer to Figure 1): The drum *A* covered with glazed paper is placed so that the lower part of its spindle is held by the clutch *B*. The burner *H*,

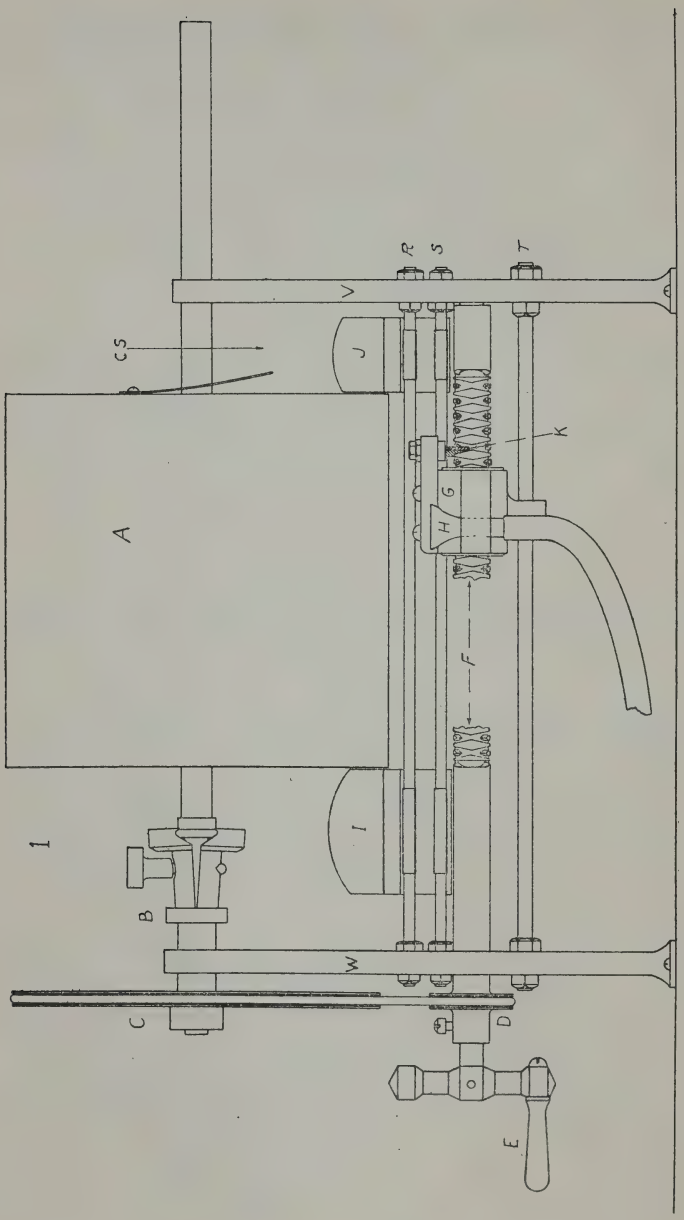
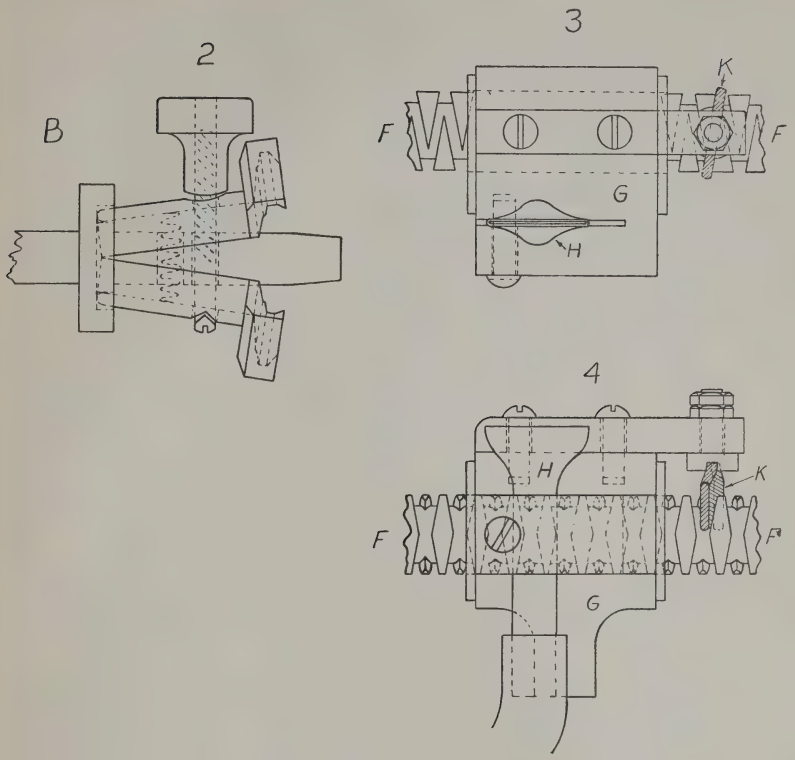



FIGURE 1

FRONT ELEVATION OF DRUM-SMOKING APPARATUS WITH KYMOGRAPH DRUM IN POSITION




FIGURES 2, 3, AND 4

FIGURE 2—DETAILS OF CLUTCH; FIGURE 3—DETAIL PLAN OF DOUBLE SCREW *F* WITH FINGER *K* AND BURNER *H*; FIGURE 4—DETAIL ELEVATION OF DOUBLE SCREW *F* AND CARRIAGE

which is under one of the paper protectors (*I* or *J*) is lighted, and the handle *E* is turned at a regular speed, about once in two seconds, so that the endless or double screw *F* and the drum *A* move simultaneously, the latter at a slower speed than the former. As the screw revolves, the burner carriage moves forward until the finger *K* arrives at the end. At that point the finger automatically turns and causes the carriage to go in the opposite direction, until the finger again reaches the other end of the screw, while the drum continues to revolve in the original direction. The to-and-fro movement of the carriage (and its burner) continues until the glazed paper is smoked to a sufficient degree of darkness, and it is stopped when the burner is under one of the protectors (*I* or *J*).

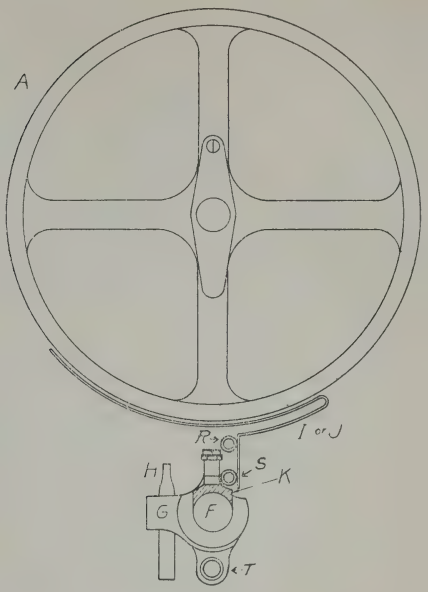



FIGURE 5

SECTION THROUGH DRUM-SMOKING APPARATUS AT POINT MARKED *CS* ON FIGURE 1

For purposes of manufacture, reference should be made especially to the detail drawings. In Figure 1, three items remain to be explained, *R, S,* and *T*. *R* and *S* are tie rods between the uprights, *V* and *W,* which support the drum and the revolving screw. These tie rods also permit a horizontal adjustment of the protectors, *I* and *J,* so that the ends of the papers covering the kymograph drum shall not be burned. This adjustment of the protectors obviates the necessity of too accurate placement of the drum on its shaft in relation to the upright supports *V* and *W*. The third horizontal rod *T* helps to keep the burner carriage firm. The positions of these rods in relation to the other parts are shown in Figure 5.

Figure 2 shows details of the "clutch." This has been constructed primarily for use in connection with the Harvard kymograph. The hollow shaft of the drum fits over the spindle and the base of the shaft is held tightly by screwing the large thumb nut which is indicated. A spring (also shown) is used so that when the clutch is unscrewed it will open widely to permit the ready release of the drum shaft. The clutch is easily changed to fit the spindle of any other kymograph drum, e.g., the Zimmermann.

Different views of the double-cut-thread shaft *F* are shown in Figures 3

and 4. The relations to it of the carriage *G* and the movable finger *K* are also shown in the same drawings. The fish-tail burner *H* is also shown. The gas supplied to this institution gives a smokeless flame with the minimum of air, and because of this the gas is sent through a wash bottle containing benzene or benzol before it reaches the burner. This gives an excellent, even smoke when the gas is regulated at the inlet.

The whole apparatus is placed on a wooden base. As now in use the smoker has been changed in only one particular from the indicated drawings, viz., the two pulleys (*C* and *D*) have been replaced by sprocket wheels (44 and 18 teeth, respectively), and the pulley by an endless chain.

The double-thread screw is made of steel, the movable finger is of phosphor bronze, and the remainder of the apparatus is brass.

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## APPARATUS FOR PERCEPTION OF FORCE OF MOVEMENT

For the consideration of kinaesthesis, numerous experiments are readily made on the extent of movement. More difficult to perform and less satisfactory means for performance are the experiments on the force and on the time of movement. In the work of Fullerton and Cattell (1) an apparatus of the nature of a spring balance was used for determining the perception of force. In their instrument the force was a constant function of the extent, as is found in the common spring balance. Estimation of the accuracy of perception of force in using such an instrument can be made only by comparison and interpolation of the extent-perception factor. An apparatus in which the extent-constant can be varied makes the student observer pay special attention to the factor of force, although he may not entirely disregard the perception of extent. The instrument here described has been used in the elementary course in experimental psychology, and because of the possibility of its variation in different experiments and its ease of adjustment it has been found satisfactory.

Figure 1 shows the plan and end elevation of the apparatus. *A* is the base, made of maple, on which the elements are assembled. The main elements are the extension spring *B* and the pull handle *D*. The spring is held rigid at one end on a metal angle by a screw *G*, which may be moved to and fro and made fast at any special point by the set nut. At the other end of the screw is the swivel hook to which the spring is immediately attached. To the other end of the spring is attached a lever *C*, upon which the pull is made by the subject through the handle *D*. The pull has a round handle, and it is attached to the lever by a snap. Upon

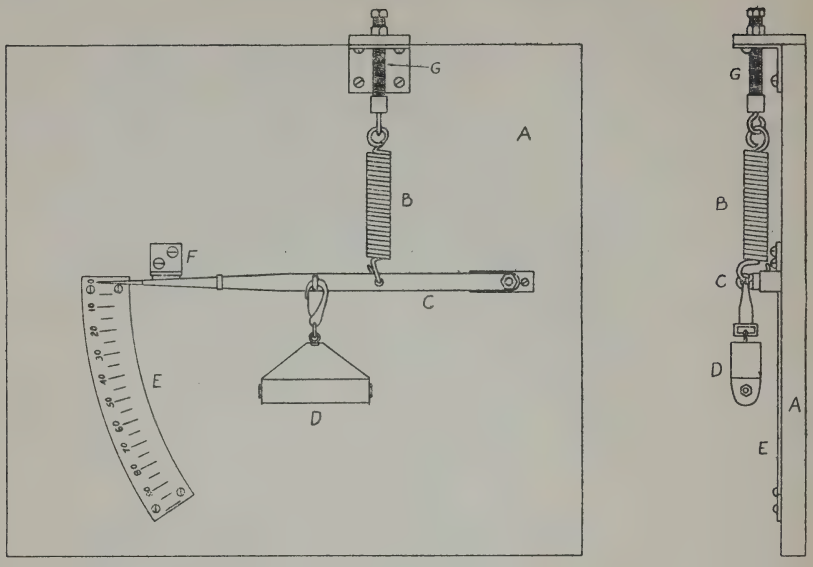


FIGURE 1

PLAN AND END ELEVATION OF FORCE OF MOVEMENT APPARATUS

the lever are two eyes, to either one of which the handle may be attached, thus changing the extent-constant for force in relation to the leverage employed. The lever is stopped by the angle stop *F*, on the face of which is a layer of soft rubber. The force of the movement (and its extent) is read from the scale *E*. The divisions are arbitrary, because the spring tension or the leverage may be changed. By turning the screw *G*, the initial tension of the spring, before the lever moves, may be changed from zero to two or more kilograms. This adjustment is made readily, and the subject may be called upon to make comparison of the force of one movement which starts in extent at zero with another movement which starts at three kilograms. Thus, the subject may be required to make an initial pull extending the spring from the beginning of the movement until he reaches the point 50 on the scale, the equivalent of about five kilograms. Then the screw *G* is set so that the spring is initially extended to three kilograms, which must be pulled before the lever begins to move, and the completion of the pull to 20 would make the two movements of the same force. Or, leaving the screw so that the spring is extended only when the pull begins, one trial may be made by attachment of the handle to one eye on the lever and the second comparison or trial when the handle is attached to the second eye. The eyes are in relation to the lever pivot

as 2:3, and in relation to the spring suspension they are 1:2.5. If the student is to be saved the trouble of calculation, a table may be constructed for definite settings of the apparatus.

REFERENCE

1. FULLERTON, G. S., & CATTELL, J. McK. On the perception of small differences. Philadelphia: Univ. Penn. Press, 1892.

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## A RECORDING PENCIL MAZE

As a class experiment in learning, the pencil maze presents a relatively complex problem, which can be mastered individually in a few hours and which has considerable collective variability. When, however, the movements must be recorded by an observer, while at the same time the observer may be busy recording whatever introspections or remarks are made by the subject, much may be lost. The apparatus shown in the accompanying drawings is a maze which in general in an extension of one originally used by Boring in his study of learning by the insane. It is constructed so that the subject (blindfolded) traces his pathway onto the paper, which is a permanent record available for the calculation of numbers of errors, or error repetitions, etc. It is also constructed in such fashion that four maze patterns are available for the same direction of the subject's movements from the starting-point to the goal.

The maze *A* is an aluminum plate, about 1/4 inch thick, cast from a pattern and subsequently smoothed and the slots filed. This plate is set upon projections in a wooden box *B,* and held in place by four lugs *C.* It is removable and may be turned through an angle of 180 degrees, or it may be turned over and used in any one of those two positions. This permits, therefore, its being used in any one of its four patterns. About 1/16 of an inch below the plate is a hard-wood slide *F,* which projects beyond the main part of the box in a handle or pull *E.* The slide can be removed and inserted easily and a sheet of paper used thereon. Below the slide are two eccentrically pivoted brass rods *D,* which are turned by the knurled nuts (also marked *D* in the plan) at the side of the box away from the subject. A third of one turn of these eccentric rods forces the hard-wood slide *F* against the lower face of the maze plate *A* and holds the record paper firm and unwrinkled when the pencil is moving over it.

In the use of the maze, a piece of newsprint paper of the same size as the slide is placed upon the latter, the thumb nuts are turned so that the paper is close to the maze plate, and the blindfolded subject begins to

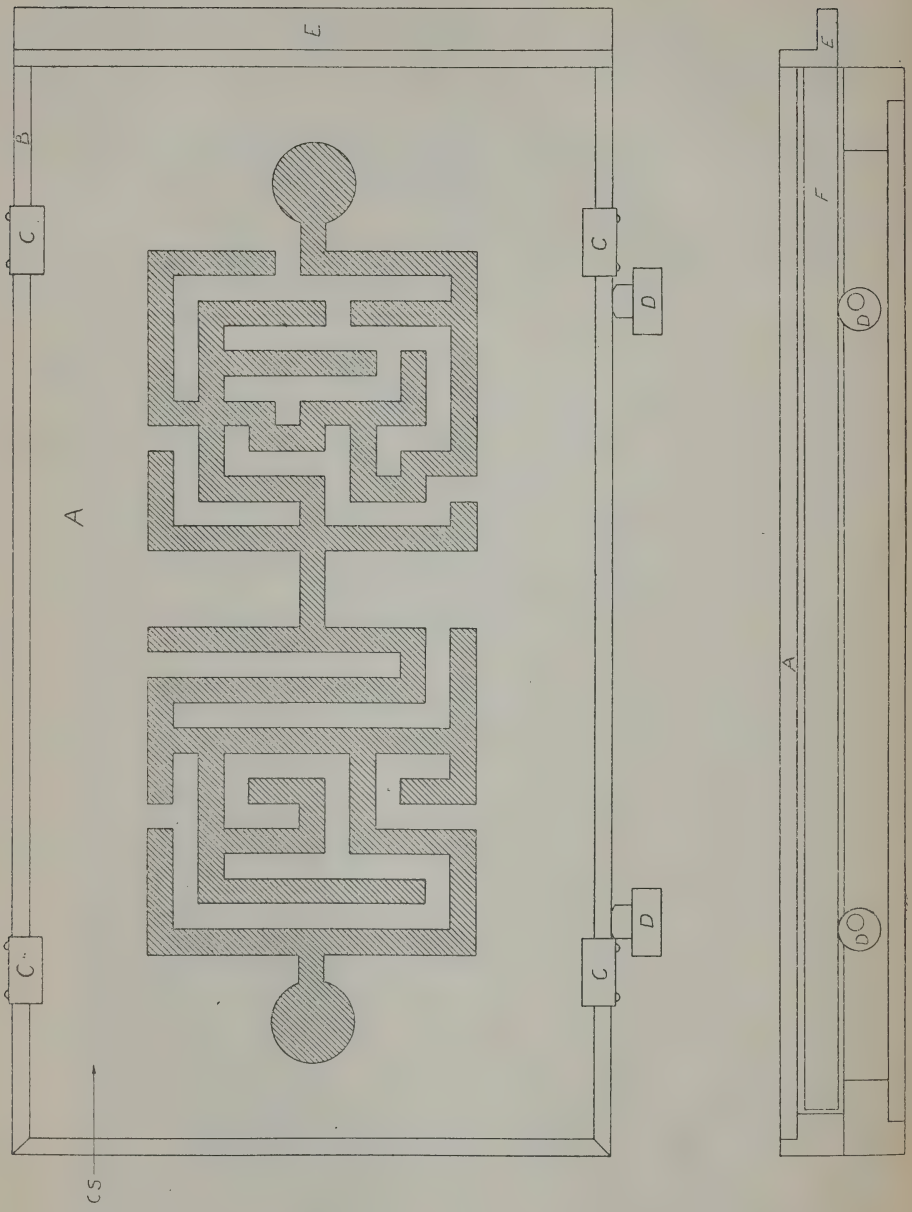


FIGURE 1

PLAN AND VERTICAL SECTION OF PENCIL MAZE

trace his pathway with a pencil. All of the correct movements and all errors are recorded, and unless the pencil is too blunt or too soft, or the subject retraces paths more than six to eight times, each effort is legible, and they are calculated after the individual experiment (or a series of experiments) is finished.

If it is desirable to exhibit general differences in the course of the individual's learning, or individual differences, to a class of about 30 students, they are evident on inspection of the records without calculation.

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## THE POSITIVE GEOTROPIC ORIENTATION OF AN ANT (*CREMATOGASTER LINEOLATA*)

The "muscle tension theory" of geotropism is found applicable to a growing number of diverse organisms (4) and the present paper confirms the a priori possibility of the rational success of this conception when extended to the study of the higher Hymenoptera. The prevalent illusion that a tropism is intrinsically "primitive" is quite unpardonable, for the first quantitative expression of geotropism was obtained in a mammal (5). The behavior of ants has elicited the curiosity of mankind from time immemorial, but the burdened literature affords not a single contribution of a thoroughly quantitative character.

No review of previous studies on the conduct of ants will be found in this paper, for the results of other investigators have been invariably obscured by notions regarding the alleged "psychic" factors. We have not as yet observed any indications of a psychic life of ants and we shall refrain from postulating such factors for we have adopted Wasmann's (8) philosophy, *"Entia nón sunt creanda sine necessitate,"* although perhaps not quite as Wasmann intended. Loeb (6) recognized the phototropism of the winged adult ants, but with curious inconsistency he ascribed "associative memory" to the workers and was unable to obtain a single tropistic response with them. It is difficult to detect evidence of geotropism in the published accounts of the inevitable "learning" experiments, but those of Turner (7) require comment.[1] Turner "trained" ants to descend inclined strips of cardboard (3 x 30 cm.) to reach the nest;

[1]It would be impossible to do justice to the appalling amount of anecdotal literature pertaining to ants. One example will suffice. R. de Gourmont watching ants crawling on a vertical surface concluded that these creatures were too small to be influenced by gravity (2)!

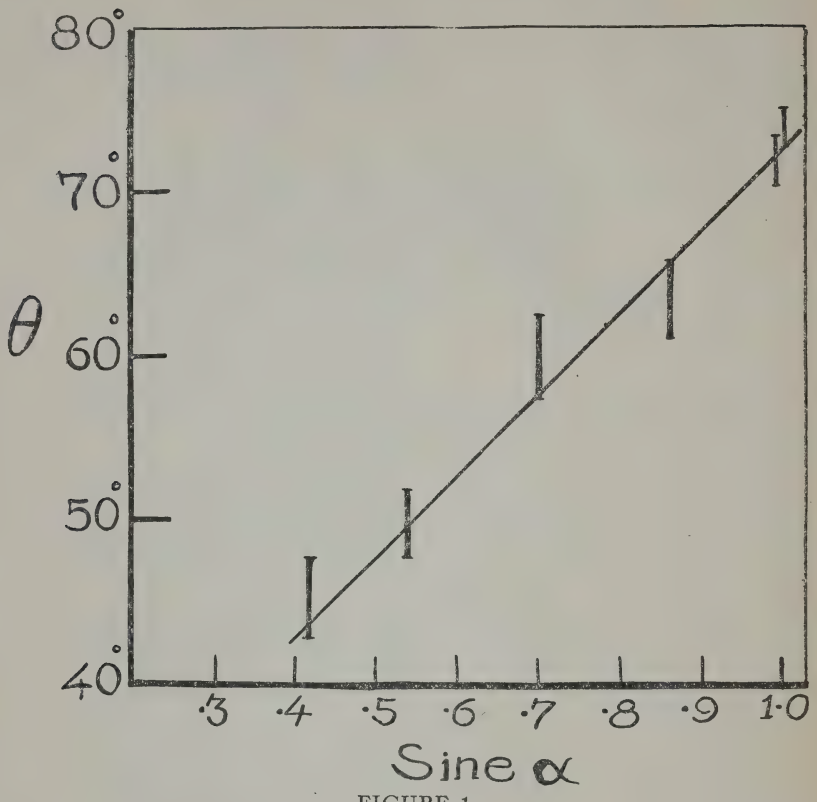


FIGURE 1

RELATION BETWEEN THE MEAN ANGLE OF ORIENTATION Θ AND THE SINE OF THE ANGLE OF INCLINATION OF THE SUBSTRATUM $\alpha$

The bars are proportional to the probable error. Each value of Θ represents the mean of 40 measurements.

then, on raising the nest, he found that the animals "often took an unusually long time to learn the way home up-hill." Cornetz (3, p. 144) in his fascinating little volume on the explorations of ants proposed a most extraordinary theory to account for the path described by an ant when traversing a roof-shaped obstacle. The ant maintains the same Θ angle (the $\alpha$ angle of Cornetz) on the incline and decline in order to preserve its "direction"; the ant is, as Cornetz (3, p. 145) puts it, *"comme lancée en droite ligne."* The successful repudiation of this untenable hypothesis demands a metaphysical consideration of the nature of absolute space, which is unfortunately irrelevant in a modern scientific journal. The

FIGURE 2

REPRESENTATIVE RECORDS OBTAINED BY FOLLOWING THE MOVING ANT WITH A SOFT PENCIL AT VARIOUS INCLINATIONS OF THE SUBSTRATUM

A ten centimeter scale is given at the left. The light straight lines are the fitted slopes. The path begins at *X*. *A*, $\alpha$ 90°. This represents the nearest approach to a rectilinear trail. *B*, $\alpha$ 82°. The first observation made with individual No. 1 (used throughout the experiment). This "eccentric" creeping soon vanishes. *C*, $\alpha$ 25°. The path exhibits wide oscillations, but the positive geotropism is quite evident. *D*, $\alpha$ 45°. A path intermediate between *A* and *C*.

TABLE 1

ORDER IN WHICH THE RECORDS WERE MADE FOR EACH OF SIX INCLINATIONS OF THE PLANE ($\alpha$)

| $\alpha$ | Time |
|---|---|
| 82 | Few readings April 4th, 10 P.M.; others April 5th, 8 P.M. |
| 90 | Immediately following |
| 45 | April 5th, 10 P.M. |
| 33 | Immediately following |
| 60 | April 6th, 8-9 P.M. |
| 25 | April 6th, 11 P.M. |

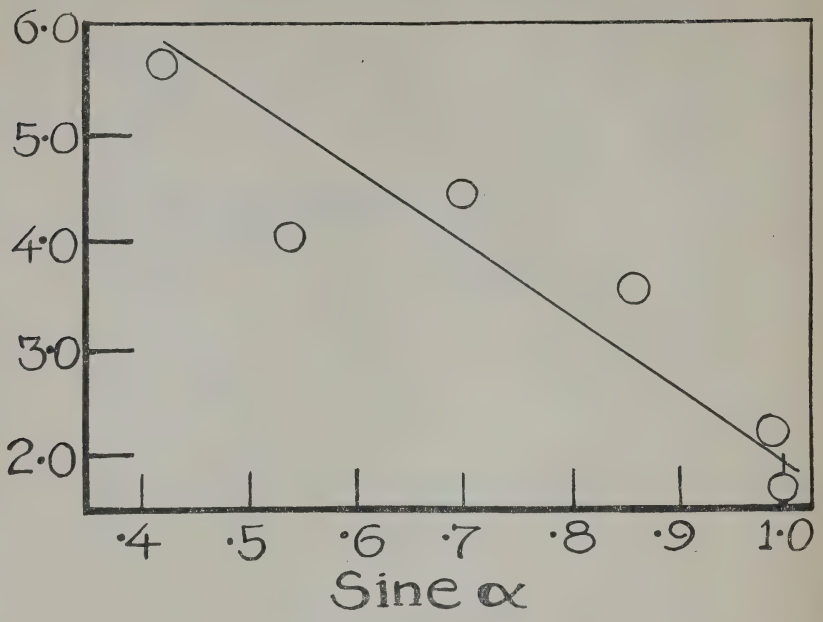


FIGURE 3

THE RELATION BETWEEN THE "COEFFICIENT OF VARIATION" AND SINE $\alpha$

The ordinates represent the probable error as per cent of the mean. The variability of the angle of orientation decreases as the active gravity component increases.

splendid monograph by Brun (1) on the space orientation of ants, while dealing especially with factors in the alleged "homing instinct," presents valuable data pertaining to the *"kinaesthetische Engramm."* Unfortunately, the inclined table used by Brun contained the nest and a source of honey, but his evidence clearly demonstrates that *Formica rufa* is responsive to an inclination of the substratum as low as 20°.

The workers of *Crematogaster lineolata* whose behavior on an inclined plane is described in this paper were collected at Forest Hills, Massachusetts, in October, 1928, and kept in a large colony (with winged virgins) in the laboratory. House flies and honey were supplied occasionally. The experiments recorded were performed on the fifth and sixth of April, 1928, in a dark room under a dim red light at a temperature of 22°C. The ant was followed on a sheet of paper with a soft pencil. The texture of the paper enabled the animal to crawl readily even at 90° inclination. Immediately after leaving the colony for the first trail a worker may exhibit very eccentric crawling (Figure 2B), but when next placed on the inclined plane a typical $\Theta$ value is obtained. The ant is lifted gently to the center

TABLE 2

MEAN VALUES OF THE ANGLE OF ORIENTATION Θ AT SEVERAL INCLINATIONS OF THE SUBSTRATUM α

$n = 40$

| α | Θ | P.E. | P.E. x 100 / mean |
|---|---|---|---|
| 25 | 45.3 | 2.57 | 5.67 |
| 33 | 49.9 | 2.04 | 4.08 |
| 45 | 60.0 | 2.65 | 4.41 |
| 60 | 63.5 | 2.25 | 3.54 |
| 82 | 72.2 | 1.69 | 2.20 |
| 90 | 74.2 | 1.25 | 1.68 |

of the plane on a bit of paper and permitted to crawl off in any direction. In most cases the animal creeps downward after a few oscillations (Figure 2D). It is impossible to record every minute turn with the pencil, but the paths are accurate enough to show the gradual decrease in amplitude of the oscillations as the gravitational pull increases. In Table 2 will be found the mean values of the angle of orientation Θ measured as deviations from a line at right angles to the longitudinal axis of the plane; α indicates the angle of inclination of the substratum.

Each Θ is obtained as the average of 40 measurements of short sections of the path; the method of obtaining this is indicated in Figure 1, A, C, and D. The order in which the records were taken is shown in Table 1, for studies of *Aphaenogaster* have demonstrated that previous crawling on the plane may increase the Θ value.

It will be seen in Figure 1 that a rectilinear relation obtains between the angle of orientation and the gravitational component or sine of the angle of inclination of the plane. The angle described by the positively geotropic ant is determined by the weight of the insect, producing tension on the upper side of the supporting musculature; this tension gives rise to proprioceptive effects leading to enhanced activity chiefly in the limbs of the opposite side. Thus any deviations from the typical Θ tend to be checked until the difference in the "pull" on each side of the body is reduced below a threshold value. The probable error of Θ (from Peter's formula) and the "coefficient of variation" decrease as the gravitational component increases (Figure 3).[2]

It would be premature to elaborate a theory of the homing of ants based

[2]The precision of the orientation also increases in quite another way; the amplitude of the oscillations of the path decreases, which may be regarded as the elimination of "random movements." Further results with *Lasius flavus* will be considered in another paper.

on the demonstrated occurrence of geotropism; our task is the more humble one of showing that geotropism occurs in these insects and may be expressed in a quantitative manner. Wheeler in his monumental work on ants (9, p. 515) points out the significance of a tropism reversal but adds with characteristic sagacity, "But this is, perhaps, too simple an explanation."

Summary

The worker of the ant *Crematogaster lineolata* is positively geotropic. This gravity response may be expressed in terms of the "muscle tension theory."

References

1. Brun, R. Die Raumorientierung der Ameisen. Jena: Fischer, 1914. Pp. 234.
2. Cornetz, V. Le sentiment topographique chez les fourmis. *Rev. des idées,* 1909, **6**, 452-458.
3. ————. Les explorations et les voyages des fourmis. Paris: Flammarion, 1914. Pp. 192.
4. Crozier, W. J. Tropisms. *J. Gen. Psychol.,* 1928, **1**, 213-238.
5. Crozier, W. J., & Pincus, G. Tropisms of mammals. *Proc. Nat. Acad. Sci.,* 1926, **12**, 612-616.
6. Loeb, J. Comparative physiology of the brain and comparative psychology. New York: Putnam's, 1902. Pp. 309.
7. Turner, C. H., The homing of ants. *J. Comp. Neur. & Psychol.,* 1907, **17**, 367-434.
8. Wasmann, E. Die psychischen Fähigkeiten der Ameisen. *Zoöl.,* 1899, **11**, 1-130.
9. Wheeler, W. M. Ants. New York: Columbia Univ. Press, 1913. Pp. 663.

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## A MECHANICAL INGENUITY TEST

The following device has been applied as one of a battery of tests for mechanical skill by the Department of Psychology, Stellenbosch University, South Africa.

The apparatus consists of a puzzle box and a blue-print drawing of its interior. Figure 1 shows the external appearance of the box, which is constructed of teak wood, 1/2 inch thick. The box measures 8 by 7 inches, and is 10 inches high. The fittings, to be described, are all of brass and strongly made so as to withstand fairly rough treatment.

The inner construction of the box can be seen in Figure 2. The rods and levers are all 3/10 of an inch in diameter. As seen from the outside, the

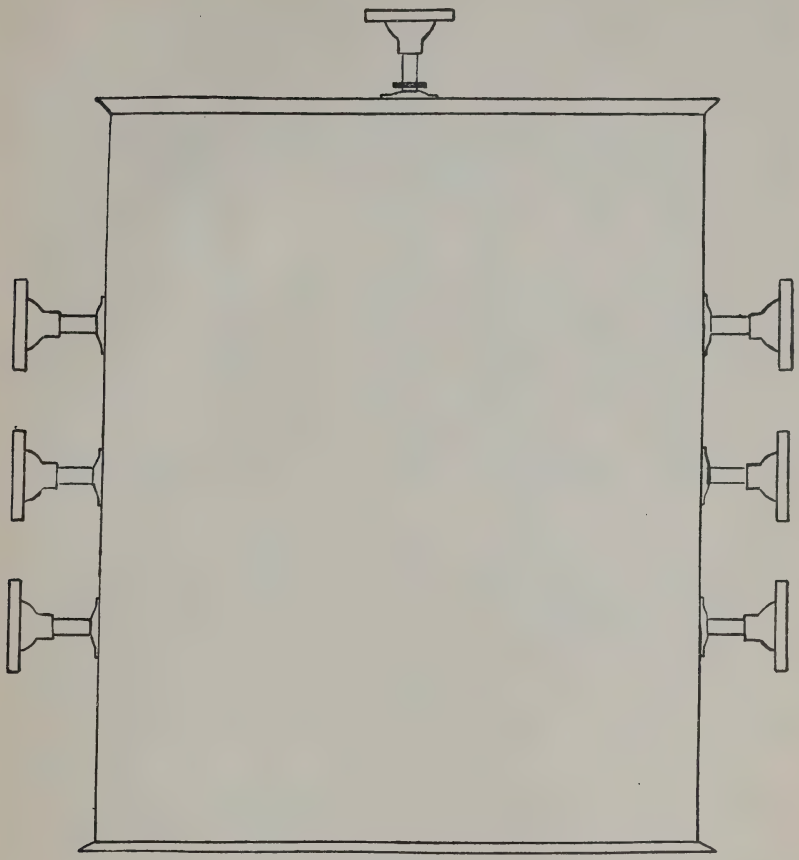

FIGURE 1

controls, with the exception of control *AH,* all appear exactly similar, and stand out 1 and 3/10 inches from the side of the box. Control *AH* differs in being fitted with an extra brass washer at *T*. All three controls on the right and left sides of the box are equally spaced perpendicularly below one another at 2 inches apart, the lowest ones being 3 inches from the base of the box. Further, they are all fitted with circular screw heads, which can unscrew if an *S* attempts to turn a control which should receive other treatment.

The rod *AH* passes through the lid and screws into the seating *R* at *H*. *Q* is a ring of 1-inch diameter, which forms part of the rod *AH*. Through this ring passes the rod *DG,* which consists of two sections, *DP* and *PG*.

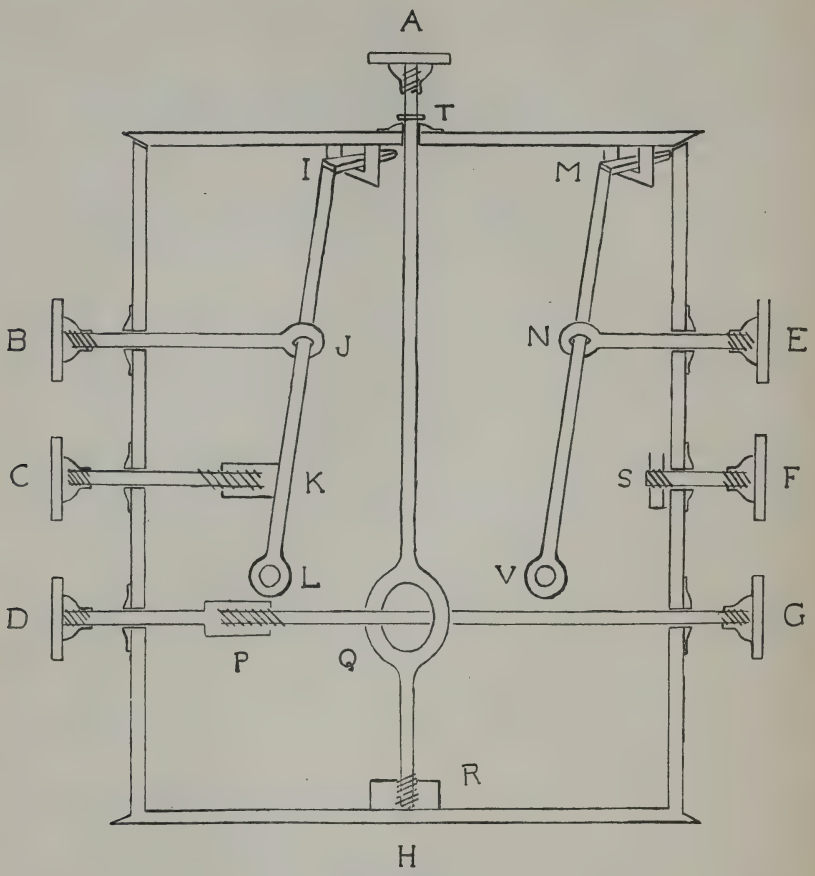


FIGURE 2

The control *DP* has a screw seating at *P*, while the rod *PG* is fitted with a screw end at *P*. When *DG* is fixed in position, it effectually locks the control *AH*, while the washer at *T* prevents the alternative of unscrewing the screw head at *A* and slipping the lid over the remaining rod.

Attached to the lid are two slots, *I* and *M*, set parallel to the sides of the box, but depicted as in Figure 2 in order to make the essential mechanical relations involved more easily comprehensible from the drawing. Slot *I* is fitted 1 and 9/10 inches from the left side of the lid; slot *M* is 5/20 of an inch from the right side. The catches of the levers *IL* and *MV* fit into these slots when the box is closed. The lever *IL* turns on a swivel at *L*. Similarly, the lever *MV* turns on a swivel at *V*. Both these levers are

5 inches long, while the catches at *I* and *M* measure 1 1/2 inches. The lever *IL* can be manipulated from outside by means of the controls *BJ* and *CK*. A screw seating is welded on to the lever at *K*, into which the rod *CK* screws. The control rod *BJ* is fitted with a collar at *J*, in which the shaft *IL* moves freely. In the same way *EN*, the control rod governing the lever *MV*, is fitted with a collar at *N* through which the shaft *MV* passes. The swivels at *L* and *V* are situated 1 and 8/10 of an inch from the side of the box nearest to them. The dummy control *FS* is kept in place by a lock nut at *S*. It is so attached that the whole fitting can turn freely.

This closed box, as described, is placed before the subject, and it is explained to him that certain catches inside the box keep the lid fixed in position, and that these catches are controlled by the levers projecting above and on each side of the box. The subject is then given a schematic blue-print drawing of the box seen in cross-section (as in Figure 2), and he is instructed to open the box as quickly as he can. By referring to the blue print, which remains before him during the test, he is able to ascertain what the mechanical relations are between the levers and the catches.

The time taken to carry out the test successfully serves as criterion of the *S*'s mechanical ability; secondly, the number of erroneous handlings of the levers, as described below, afford a further measure of efficiency.

The correct procedure in opening the box is as follows:

1) While lever *D* is held fast in the left hand, *G* must be unscrewed, and then drawn out as far as it will go. The apparatus is so made that, if the subject omits to prevent *D* from moving, the entire shaft *DG* will turn when the attempt is made to unscrew *G*.
2) Next *C* must be unscrewed and drawn out. This operation unlocks *BJ*,
3) which can now also be drawn out.
4) *E* must be pushed in flush with the side of the box.
5) *F* remains untouched (being the dummy).
6) Finally, *AH* must be screwed loose and the lid lifted.

It will be seen from the illustration that the order given need not be adhered to in all respects but, also, that the different steps cannot all be carried out in any order. When *CK*, for instance, is still in place, *BJ* is effectually locked. Similarly, the shaft *AH* is locked by *DG*.

Under erroneous attempts is included:

1) Premature attempts to force the lid off.
2) Manipulation of the dummy lever *FS*.
3) Attempting to turn control *BJ* or *EN*.
4) Attempting to draw out control *EN* or push in *BJ*.
5) Attempting to unscrew *AH* prematurely.
6) Attempting to draw out *BJ* prematurely.
7) Attempting to force levers *AH*, *CK*, or *DG* in or out instead of unscrewing them.

One hundred and sixty boys, averaging 14 years of age, were tested with this apparatus at the Johannesburg Trades School. The test was carried out in the following way:

1) First attempt—instructions were given to open the box with the blue print as reference.
2) Immediately after the successful completion of this task, the *S* was instructed to reclose the box with the blue print as reference.
3) Thereupon the *S* opened the box again, now without reference to the blue print.
4) The *S* had finally to reclose the box without the help of the blue print.

It will be seen that in the reclosing of the box, the order of the different operations is reversed.

AVERAGE TIMES TAKEN

| | | |
|---|---|---|
| 1 | 4 min. 5 sec. | First application |
| 2 | 3 min. 16 sec. | Second " |
| 3 | 1 min. 7 sec. | Third " |
| 4 | 1 min. 45 sec. | Fourth " |

Ninety of these boys have recently written a Government Preliminary Technical Examination. This test is mainly theoretical but includes practical geometry and drawing. The other subjects examined are: language, trade theory, chemistry, mathematics, and physical science.

If we divide these 90 boys into two groups, according to their success or non-success in the examination, we find that the average time taken by the first group for the opening of the mechanical test box is 3 min. 50 sec. The second group's average time is 5 min. 27 sec.

The teacher's opinion regarding the boy's workshop ability shows with the first application of the test a correlation of .36. Although this result is not as satisfactory as one would like, it is partly due to the fact, we hold, that the boys had not all been at the school for the same length of time. Further, it was found that the type of work done at the school did not in all respects incur the particular kind of mechanical insight presumably tested by this test.

The further applications (2, 3, and 4) gave no agreement with examination results or the teacher's judgment of the boy's ability. Those boys who took over 20 minutes in opening the box the first time take a very short time in reclosing it; others who spend a very short time in opening the box take a longer time to reclose it. This is due, we hold, to the fact that those who take a long time on the first form of application had, in so doing, a better opportunity of studying the apparatus, and obtaining a better knowledge of its internal arrangement.

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## A UNIVERSAL FINGER MAZE: THE PATTERN BOX[1]

### Maze Pattern and Maze Construction

At this stage of development when maze work itself has hardly come out of its own blind alley, it is highly profitable to distinguish *maze pattern* from *maze construction.* The difficulty of a maze is not only a function of the pattern but also of the material or manner in which it is constructed. While the controversy over the reliability of maze work (4, 1, 3) has resolved itself into a question of pattern (10) and statistical considerations (11, 12), the influence of construction upon the results of learning can hardly be overemphasized (7, 5). Two types of construction for all manual mazes for humans are now available, namely, the old depressed stylus or groove type and the new elevated "finger" type (7, 8). The difficulty of construction,[2] the objection of running along one side, and the cost of construction will surely yield the former to the latter for general usage. Vincent's innovation (13) of taking away the walls of the usual alley type for studying the function of vibrissae in the behavior of the white rat might have been the forerunner of the later elevated type of construction, but it was through the deliberate effort of Professor Miles that the promised elevated type of construction as a new technique was definitely established.

To be sure, this elevated type of wire construction is much more readily made, can be easily duplicated, lends itself to the making of changes, can be more complex, and, above all, can be traversed much more rapidly than the former depressed type of groove construction. But with the *discrete-path* variety described in this paper, by the use of shots, nails, balls, and pins on a perforated metal screen or a wooden board, any pattern can be built or destroyed at will without disturbing the unit itself, and hence it is *universal* in nature. In comparison with this, the wire construction is much less readily made, can be much less easily duplicated, lends itself much less to the possibility of making changes, although perhaps equal complexity and rapidity can be obtained by it. The readiness for making

[1]Expenses in connection with the developing of the *pattern box* were met from the Thomas Welton Stanford Fund for Psychological Research. The writer is indebted to Professor W. R. Miles for criticisms and suggestions.

[2]In connection with another development called the multiple groove board described in (2), which is essentially a series of vertical and horizontal, machine-milled grooves, and is designed for testing manual skill in controlling a rolling ball along prescribed grooves, a simple method for constructing a groove maze was suggested. It consists of nailing pieces of wooden blocks on a panel base along all the boundaries of the maze pattern so that walls are formed by the blocks on each side of the path. Professor Miles has actually adopted this method of constructing the groove maze (6) in connection with a comparative study between the stylus and the finger maze.

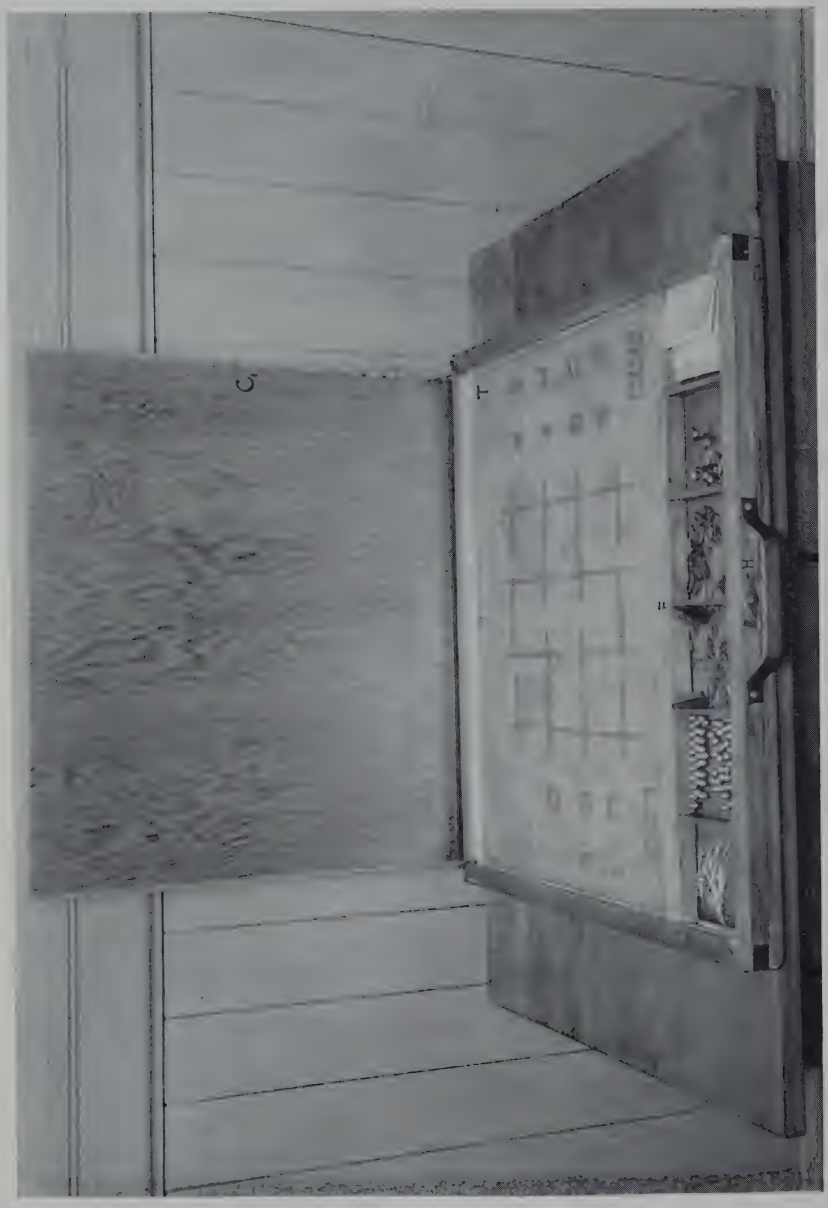

FIGURE 1
THE PATTERN BOX: A UNIVERSAL, DISCRETE-PATH, FINGER MAZE BOX

a pattern in the wire construction is perhaps not so obvious as it first seemed to be in description (7). The panel has to be, first, specially cut and sandpapered; the design or pattern has to be marked accurately and proportionally by pencil on the surface before the wires can be nailed into the marked units. The cutting of the wire requires a special cutter and some previously trained skill, not to say the delicate judgments of the different lengths of the units and the careful bending of the two ends for engaging into the wood. No less skill is involved in the hammering of the units into the wood. After the different units are completely inserted, the entire surface has to be varnished to prevent it from getting dirty through constant tracing by the finger. Thus at least one day is required to build any one pattern, and duplication means, of course, going through the entire procedure once again. Once made, the pattern practically lends no possibility to make changes (contrary to what the first description (7) seemed to contend because the removal of any unit would certainly leave a hole at each end and hence make the floor not uniform.

The writer's interest in the development of a discrete-path, universal-construction variety of the elevated finger maze began in the fall of 1926 when he first saw a demonstration by Professor Miles of this type made from narrow square oak strips glued to a panel. The writer was aware of the possibility of such a variety and was convinced then that, when the old Hampton Court diagram was first used by Small (9) as a rectilineal maze for animals, probably it never occurred to anybody that "maze" would become synonymous with animal learning, that maze work itself has come to its own blind alley, and even its usefulness as an instrument of research in animal learning (4, 1, 3) has been doubted, that this doubt cannot be cleared unless standardization of the apparatus (14) is perfected and evaluation of the merits of different maze patterns (10) is carried out, and that its positive value and usefulness as an instrument of research on behavior of both human and infra-human subjects cannot be established unless some convenient piece is devised that is really easy to set up and universal in nature so that a comparative study of the different existing maze patterns can be made on human subjects to ascertain their relative merits all at once. The *pattern box* together with some related varieties of universal, discrete-path, finger maze-construction described in this paper was a direct outcome of this awareness and conviction.

### Some Varieties of Universal Maze-Construction

The writer started with the original idea of using small ball bearings of 3/16″ diameter to make the maze pattern on a piece of 3-ply panel of white cedar, in which a series of holes of 1/4″ diameter in the shape of a half bowl was drilled just deep enough to hold the balls in position. When not in use, the balls were kept in their respective holes, making the maze pattern by a sheet metal cover. As holes were in rows and columns all over the board, any vertical or horizontal path could be formed instantly.

This is the *first* design. A *second* design was to drill the holes from the bottom of the board but not quite clear through with the drill so that the balls may be inserted from below and kept inside the holes by a piece of thin metal plate or wooden board placed below. This will keep the maze pattern relatively permanent because the balls can neither fall off from above nor slip off from underneath. A *third* design was to drill two duplicate plates of tin sheet 1/16″ thick exactly alike, the diameter of the holes being just a trifle smaller than that of the ball so that when the maze pattern is made the two plates will keep all the balls between them by bolts on the four corners. This design gives the extra advantage of obtaining the mirror effect of the maze pattern by simply turning the whole apparatus over and using the back pattern.

The difficulty of drilling the holes, either in the wooden board or in the metal plate, small and close enough to make the path reasonably continuous and the failure to find some ready-made material that will fulfill the requirement led the writer to make use of ordinary brass-plated upholstery nails. The first thought was some sort of tack with a round smooth head and a stem; then Professor Miles suggested this particular nail, which is about 14 mm. long with a round smooth cap of about 8 mm. in diameter. Double corrugated cardboard and 1/4″ plain linoleum were tried for the base of the maze pattern. The nails can be pushed in just like thumb-tacks so that building or destroying a maze pattern is a matter of minutes. The appeal of this design is its simplicity and cheapness of construction besides the universal nature of maze pattern.

These designs were all more or less preliminary experimentations to the perfected universal, discrete-path, finger maze-construction called the *pattern box*. The name was chosen because of its many possibilities both as a psychological apparatus and as an educational instrument.

## The Pattern Box

After quite a little further search for a better material than the brass upholstery nails and for some ready-made perforated plate for the pattern base, the usual commercial brass escutcheon pins (size No. 14 with a head diameter of about 2 mm.) were found best for the purpose together with the ready-made perforated tin sheet that is used in all metal works for housing instruments that liberate considerable heat. The perforated tin sheet comes 14″ x 20″ at the cost of about 60 cents a piece (retail price). Any size of holes may be obtained. As the perforations become larger, the distance between them becomes proportionally greater. For No. 14 escutcheon pins, a plate with ten perforations to the inch just fits.

It is obvious that any number of universal, discrete-path maze-boxes can be very conveniently made by putting a wooden frame around the four sides of the perforated tin sheet with a sliding cover to keep the pins in position and be used in each laboratory over and over again, thus avoiding

the trouble of building a separate maze board for every maze pattern as in the case of wire construction. The discrete path formed by the pins on the perforations has all the characteristics of the wire path and can be altered instantly. A special pin picker bent out of a piece of flexible brass makes it possible to pick a row of pins out of the perforated screen all at once.

The *pattern box* was an elaboration of construction of this sort of universal, discrete-path, maze box. As shown in Figure 1, the whole box is a neat, portable, flat case, which measures 19½" x 20½" x 1⅜". It was designed to be used on both sides. The two panel covers (*C*1, *C*2), which slide easily along the grooves just like an ordinary box cover, here serve four distinct purposes; namely, as covers for the box, as protector of the patterns on the screen *T*, as bottom for the six compartments for storing pins and other accessories, and as a screen for keeping the pattern for experiment away from the view of the subject as shown in the position of *C*1 by the aid of two S-shaped hooks. When not in use, the two covers are locked to the box by the S-shaped hook *H* and keep both the pattern and the accessories all securely inside the box. The extra inner frame *F* serves both as the fourth side-frame for the perforated screen base and as the other frame for the compartments.

On the screen are shown some of the illustrative sample patterns. The modifiability and appeal of the whole design can be appreciated from the fact that designs or patterns as small and complex as the letters of the alphabet can be built on the screen. The big size upholstery nails with rough or smooth heads were used as the start or finish of a maze pattern.

One exceptional advantage of this universal, discrete-path maze-construction over the panel-board, wire construction is that it lends itself also to the possibility of using the same kind of wire which Professor Miles uses in building continuous path units by engaging the two bent ends into holes wider apart on the screen. A convenient and neat arrangement was to cut ordinary Gem paper clips No. 1 in two and to hook the two ends of the staples thus made out of the resulting halves on the perforations of the screen.

As the pattern box is perhaps too elaborate for ordinary use, some suggestions for modified forms are here given. (1) A single sheet of the perforated screen can be very conveniently and cheaply made by furnishing four frames and a cover. (2) The cover and protector can be made to swing upon a hinge, like opening the leaves of a book, instead of sliding along the grooves. (3) The perforated screen can be made separate and detachable so that different ones with different sizes of perforations may be used as desired.

The possibilities of the pattern box are many. It can be used for picture-completion tests and performance tests as suggested by Terman, for spot

pattern tests by McDougall, for tactual experiment on Gestalt by Koffka, for a toy for children, and for the use of blinds by Miles, although Tolman was content to use and own it for general maze experiments for which indeed it was originally designed. It may be used also as a motor test. Artists, designers, and blind readers perhaps can make great use of it. At the suggestion of Professor McDougall, the Stoelting Company has been approached to put it on the market.

The writer anticipates with keen interest that, with the wire technique of maze construction as originated by Professor Miles and with the universal, discrete-path construction as described in this paper, maze work will take a new direction to throw new light on the validity and reliability of the "maze" as an instrument of research on both human and infra-human subjects.

## References


1. Carr, H. A. The reliability of the maze experiment. *J. Comp. Psychol.,* 1926, **6**, 85-94.
2. Chou, S. K. A multiple groove board for testing motor skill. *J. Exper. Psychol.,* 1929, **12** (in press).
3. Hunter, W. S. A reply to Professor Carr on the reliability of the maze experiment. *J. Comp. Psychol.,* 1926, **6**, 393-396.
4. Hunter, W. S., & Randolph, V. Further studies on the reliability of the maze with rats and humans. *J. Comp. Psychol.,* 1924, **4**, 431-442.
5. Husband, R. W. Human learning on a four-section, elevated finger maze. *J. Gen. Psychol.,* 1928, **1**, 15-28.
6. Knotts, J. R., & Miles, W. R. Notes on the history and construction of the stylus maze. *J. Genet. Psychol.,* 1928, **35**, 415-427.
7. Miles, W. R. The high relief finger maze for human learning. *J. Gen. Psychol.,* 1928, **1**, 3-14.
8. ————. The narrow-path elevated maze for studying rats. *Proc. Soc. Exper. Biol. & Med.,* 1927, **24**, 454-456.
9. Small, W. S. Experimental study of the mental factors of the rat, II. *Amer. J. Psychol.,* 1900-1901, **12**, 206-239.
10. Tolman, E. C., & Nyswander, D. B. Reliability and validity of maze measures for rats. *J. Comp. Psychol.,* 1927, **7**, 425-460.
11. Tyron, R. C. Demonstration of the effect of unreliability of measurement on a difference between groups. *J. Comp. Psychol.,* 1928, **8**, 1-22.
12. ————. Effect of the unreliability of measurement on the difference between groups. *J. Comp. Psychol.,* 1926, **6**, 449-453.
13. Vincent, S. B. The function of the vibrissae in the behavior of the white rat. *Behav. Monog.,* 1912, **1**, No. 5. Pp. iv+85.
14. Warner, L. H., & Warden, C. J. The development of a standardized animal maze. *Arch. Psychol.,* 1928, **15**, No. 92. Pp. 35+14 plates.


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## FEELING AND KNOWING

In surveying the current psychological literature, including both articles and textbooks, one is impressed by the remarkable longevity of the age-old antithesis between feeling and knowing. Remnants, even more than remnants, of the faculty psychology are still with us. It cannot be said of this particular speculation that the good die young. It is not young, and it is not dead. It was at first probably a semilogical distinction before the days of stimulus and response, process psychology, controlled introspection, and behaviorism. The fact that it now lingers with us is evidence that even psychologists are quite human and, like other humans, are frequently the followers of tradition. As we grow older, changing fashions appeal to us less and less, for there are fashions in science as well as in clothes.

From the exclusively introspective aspect there may certainly appear to be differences, but, if we view behavior in an objective way, without becoming technical behaviorists—if that is a sin—then it is apparent that the only real difference or distinction between feeling and knowing, to use a trite expression, is one of degree, not of kind. The difference or differences must be expressed ultimately in terms of quantity, not of quality, for all scientific explanations must be in quantitative terms, meaning by that in terms of mathematics. All qualitative differences are in the last analysis quantitative differences. This does not mean the unreality of the qualitative, but that there is, so to speak, a common denominator enabling us to make measurements and comparisons.

Feeling represents a vague, undifferentiated, undefined, indefinite, and primitive type of response to a rather indefinite, vague, hazy, and unanalyzed situation. Knowing, on the other hand, in the sense in which this term is used, is simply a more detailed behavior of response. It is more definite, less vague, less primitive, more adequate, and all this is made possible by more detailed and more complex structures as found in the receptors, conductors, and effectors of organisms which have reached a certain stage of development. Thus these organisms are able to analyze their environment. The more complete the analysis, the more exact the adjustment in the form of response, in other words, the more complete the knowing. Knowing and feeling, therefore, are to be compared in terms of behavior, measured in terms of amount. There is a gradation of responses from what we call feeling through instinct or emotion until we reach knowing and what is called rational behavior.

This position is in harmony with the action theory of consciousness so well expressed by Münsterberg. It is in agreement, too, with the biological conclusion that the higher or newer structures include the lower or more primitive, and also with that theory of instinct which states that the drives are protopathic and the expression epicritic, with the additional qualifica-

tion that feelings are more primitive than emotions and instinct.

If there is some truth in this position, then there is an explanation why feelings accompany or form a part of all other forms of knowing and why some psychologists have listed them as sensations of an elementary sort. Even the mere attraction to a stimulus or withdrawal from it constitutes an elementary form of knowing which, as we know, accompanies the higher process, which is traditionally called cognition. It may be, too, that between these two extremes a place can be found for what is called insight and sagacity.

The order of precedence, then, would be something after this fashion—feeling, emotion, cognition, on the so-called subjective side; and attraction-repulsion, instinct, rational behavior, on the so-called objective side. As further evidence that the higher forms of knowing include the lower, consider human behavior in times of crises, emergencies, etc. There is a reversion to the lower when no other or higher appears to fit the situation. Perhaps it is incorrect to speak of higher and lower. Simple and complex would probably be more adequate terms. Thus the primitive is always with us. In all forms of knowing or behavior, feelings and emotions are present. They are part and parcel of all knowing and there is really no antithesis, as some belated thinkers would have us believe. Knowing is thus a rather homogeneous process to be taken as one whole, and the various stages are to be regarded as differing more or less in complexity.

Feelings and emotions are thus simpler forms of cognition corresponding to simpler response systems, which, of course, as we know, are included in the higher response systems. If we acknowledge the biological principle, how can we exclude the psychological, if structure and function go hand in hand through the ages? All of our behavior can really be divided into two classes: (1) behavior towards a stimulus; (2) behavior away from a stimulus. Our behavior in the intellectual sense is nothing more or less than an extension of the more primitive forms. There is more manipulation of environment due to more complex structures in receptors, conductors, and effectors. In kind it is not different from the primitive, and should not be held in contrast as if it were something different. Knowing belongs to the same continuum as feeling.

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## ONE HUNDRED CASES OF COLOR-BLINDNESS DETECTED WITH THE ISHIHARA TEST[1]

For two years, 1927 and 1928, at Stanford, we have used a color-vision test as a routine part of the physical examination of entering students, athletes, and others who come under the scrutiny of the Department of Physical Education for Men at the beginning of the autumn quarter. The number examined each year has been around 600. The examinations have been concentrated into four days, which means that when the system is working smoothly the men come along with their cards at the rate of about 25 per hour. If one examiner is to handle the color-vision test, he has two or three minutes to devote to each man.[2] By using the very cleverly devised Ishihara Test (6),[3] which consists of numbers in colored spots on a color-contrast background of colored spots, it is quite feasible to examine men at this rate and to secure trustworthy results on them.

We do not know just when the Ishihara Test became available for use in America or how widely it is known and used at present. Dr. Janet Clark described it accurately and fully in 1924 (4) and designated it as "the most successful of the various colorblindness tests." She compared the Edridge-Green card test, Stilling's plates, and the Ishihara plates. All three are similar. The first two are based on the fact that numbers printed in red or green and without distinct borders cannot be distinguished on backgrounds of browns, tans, and grays by the red-green blind. Therefore on Stilling's plates, for example, if the color-blind fails to see the green or red number on a plate he usually sees nothing, or perhaps guesses a wrong number from fragmentary cues. Dr. Ishihara, while making use of this same fact in his plates, designed them to take advantage of the further consideration that blue, yellow, and violet for the usual color-blind are remarkably bright as compared with red and green colors. In several of the Ishihara plates there are two numbers superimposed. The normal eye sees the green or red number, while the red-green blind fail on this but read confidently and promptly the (to the normal eye) very faint violet or blue number. Chase (3), 1926, in his discussion of methods and problems concerning color-blindness and industry, states that he has reviewed examples or descriptions of more than one hundred tests. He does not mention Ishihara, although giving attention to Stilling's plates and

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[1]Acknowledgment is made to Dr. Thomas A. Storey, Director of Physical Education for Men, Stanford University, for his kind cooperation. He informs me that the results of this work have convinced him that it is desirable to use regularly a color-vision test in their routine physical examination.

[2]It is best to make examinations by daylight between 9 to 12 A.M. and 1 to 4 P.M.

[3]Devised by Dr. Shinobu Ishihara, Professor of Ophthalmology, Medical Department, Imperial University of Tokyo.

holding that these should be used in conjunction with the Edridge-Green lantern as the standard test for color-blindness. He states further, "Stilling's plates are the standard of this group, and of great use in convincing officials, and friends of the candidate. They fail to detect color ignorance, small scotomas, and some forms of blindness, including shortened spectrum." We are inclined to think that Chase had not discovered the Ishihara Test; however, his criticism probably applies to this general type of test. Dr. Ishihara in the introduction to his test manual states that his plates are "intended to discover quickly and accurately congenital colorblindness" and that "acquired colorblindness, being of secondary significance, has been disregarded." There are, however, ways of giving these reading tests for color-blindness, e.g., varying distance, requiring color naming of the numbers, of the backgrounds, and of particular dots, and testing one eye at a time, which will bring out more than the mere fact and nature of congenital defect. But as a means of quickly and accurately discovering the latter among literate adults the Ishihara Test leaves very little to be desired.

The plates are mounted on a folded strip, which can be turned as pages of a book or stretched out and fastened up for horizontal display of the entire series. This full-display method recommends itself when a large group is to be examined. Sibyl Terman (8) used the test in this manner for selecting the 63 cases of color-blindness which she studied more intensively by other methods. The Ishihara plates were all displayed to a class of 20 or 30 university students. Each member had a mimeographed form on which to write down the numbers which he or she read from the charts. Distance was not constant, and doubtless some people were influenced by others who could make out the numbers more promptly. It is not clear how Haupt (5) used the Ishihara Test, whether as a book or with all the charts displayed. She studied 487 girls and 448 boys and gave it individually and also monocularly, but the distance is not specified.

In our own work the group of plates was attached to the wall or to a vertical screen at a height of about 5 feet and under good even daylight illumination. Two chalk lines were made on the floor, one at 4 feet, the other at 10 feet from the vertical plane carrying the charts. The men were admitted to the room one at a time and were tested binocularly. They passed along the 4-foot line parallel to the plates, reading them off one after the other. In each case, therefore, the observer was about squarely in front of the plate to be read and about 4 feet away from it. If he made no mistake or only one mistake, he was judged normal and no further testing was done. If he made several mistakes, we took his name, started him again at the first of the series, and wrote down his response to each plate. Then in many of the cases, although this was not carried out as a regular part of the procedure, the observer was asked

to step back to the 10-foot line and to make sure at this distance whether the numbers were clearer, less clear, or unchanged in their appearance.

The numbers on the Ishihara plates are about 2 inches high, a trifle more when there is but one number on a plate and a little less than 2 inches where there are two numbers on a plate. At a distance of 4 feet a number 2 inches high subtends a visual angle of about 2½°, while at 10 feet it subtends 1°. The fovea is commonly considered to have a 1° diameter, and that of the rod-free area is supposed to be close to 3°. A subject with normal color vision can easily read the plates at a distance of 10 inches, where the numbers would subtend about 11½°. If a subject fails on several of the charts at 4 feet but can read them or see the numbers much more clearly at 10 feet, it may be supposed that his color fields are extremely limited, being practically confined to the fovea.

With the aid of several assistants[4], who have helped or have conducted the examination on different days, 1286 Stanford men have been individually examined. In order to be considered a case of defective color vision, a subject had to fail on or misread two or more of the plates. There were 106 such cases, which amounts to 8.2 per cent of the group tested. Approximately one-half of these cases were found to be unaware of any color-vision defect. The same test given in the same way[5] was administered to 436 Stanford women. Out of this entire number we found one case of undoubted color-blindness. Her father also was color-blind, and she discovered her own condition at the age of 12. She misread or failed on 8 plates, but 65, that is, 61 per cent, of the color-blind men made more mistakes than did this color-blind woman. There were three other women in the 436 who made one mistake each. In every case this was on Plate 5, which the normals read as 74[6] and the red-green blind read as 21. Two of these cases read it as 21, the other gave 71. Color-blindness in women is so rare than even slight defect is probably significant and should be counted. For this reason in this survey we have tentatively counted 4 cases (0.9 per cent) of color-blindness or color-vision defect among the 436 women tested.

Our results may be compared with those of Haupt, who gives a table (5, p. 301, Table 7) showing the incidence of defect as revealed in 487 girls, age 7 to 16 years, and 448 boys of the same age range. Concerning cases entered in this table, it is stated that "except where indicated in

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[4]My acknowledgments are to Henry Beaumont, Kirk Miles, Harold C. Sox, and W. L. Roach, all of whom were employed at different times as technical assistants in these routine examinations.

[5]The women were examined by Mrs. Ruth T. Wilson, for whose assistance I am grateful.

[6]Clark says "This one card..... is a better test of color vision than all of the plates of the Edridge-Green and Stilling tests together" (4, p. 274).

brackets, all the plates in the Ishihara Test were missed. The table includes all cases failing more than one (the 5th) of the Ishihara plates." There were 8 girls and 35 boys who demonstrated defect in color vision and for each sex the cases were well distributed throughout the entire age range. Apparently 31 of the boys and 3 of the girls missed all the plates. Five girls missed part of the plates. One girl, 12 years old, missed 7 of the plates with the left eye but made not one mistake in the Nela Test with this eye. Another girl, 9 years old, made no mistake on the Ishihara Test with the left eye but made 18 out of 22 possible mistakes on the Nela Test with this eye. The 31 boys who missed all the plates show a remarkable variation in the number of mistakes made on the Nela Test. We have had no experience with this latter test and, therefore, cannot go further into a discussion of these comparison results.

Haupt gives a summary table of estimated percentages of color-blindness. This embodies results from many sources.[7] The examination has usually been made with wools or specifically by the Holmgren test. We may now add a supplementary table (see Table 1) of results with the Ishihara Test.

TABLE 1

INCIDENCE OF COLOR-BLINDNESS OR DEFECT IN COLOR VISION AS INDICATED BY THE ISHIHARA TEST

| Examiner | Year | Material studied | Cases found | |
|---|---|---|---|---|
| | | | Number | Per cent |
| Haupt | 1926 | 448 boys, 7-16 yrs. | 35 | 7.8 |
| Miles | 1927 | 590 university men | 48 | 8.1 |
| Miles | 1928 | 696 university men | 58 | 8.3 |
| Haupt | 1926 | 487 girls, 7-12 yrs. | 8 | 1.6 |
| Miles | 1928 | 436 university women | 4* | 0.9 |

*Three of these cases made only one mistake (see text) whereas all other instances in the table have made two or more mistakes.

This shows about 8 per cent incidence for men, while the typical wool test is usually credited with about 4 per cent. Similarly for women we find a higher per cent of defect indicated with the number test for color-blindness than with the wool test. Color-naming or color-matching is doubtless more simple and elemental than is the discrimination of design in color, but probably the latter is more generally important and useful. The eye in exploring the visual field usually "covers it" in unit areas considerably larger than "fovea size." We ordinarily use larger units than this even in reading. Color fields that extend well beyond the fovea are thus of great usefulness in our normal perceptions of colored objects. Defect of this character is indicated by the Ishihara Test.

[7]As recent bibliographies on color-blindness the reader may consult (1) and (2), which includes a chronological bibliography of 425 titles.

The responses of our 106 cases with the different Ishihara plates are analyzed in Table 2. At the left is the plate number. This refers to the placement in the series considered from left to right when fastened on the wall. The plates, which are 12 cm. square, are attached to a heavy strip of black paper, 19 cm. wide. Above each plate its serial number was written in white. These serial numbers were large enough to be seen at 10 feet. They aided the examiner in his work, for example, he could say, "Please read Plate 7 again." The second column shows the number on each plate that is observable to those with normal color vision. On Plates 10 and 11 the color-blind frequently see numbers, but the normal do not; for sake of simplicity we have eliminated them from the test. Plate 12 has the 2 in red and the 6 in violet-red. Also, for Plate 13 the 4 is red and the 2 is violet-red, while the background for each of these plates is gray. For convenience I have counted Nos. 12 and 13 as equivalent to two plates each. The third column in Table 2 gives the response that is usual for color-blind subjects. For Plates 12 and 13 subjects usually see the red 2 and the red 4 or the violet-red 6 and violet-red 2. The totally color-blind are not able to see numbers on any of the charts with the possible exception of No. 1. The fourth column gives the number of times that a given chart was read correctly, that is, as the normal are supposed to see it. Then we give in order the number of times that a given chart was read as the red-green blind are supposed to see it, the number of miscellaneous readings (imagined numbers), and finally the number of times no number whatsoever could be seen. The percentages are all computed on the basis of 106 cases.

From Table 2 we see that the first plate is calculated to inspire confidence at the start of the examination; everyone was able to read it. No. 2 was correctly read by half (52 per cent) of those who have color defect, and 37 per cent saw the alternate or hidden number in it, while only 3 were unable to see any outline. Somewhat more difficulty is experienced with Plate 3, which like No. 2 has a green background but more red in the number. This causes fewer to read it correctly and more to see the alternate hidden 5. There are very few who correctly read Plates 4 and 5. Here the numbers are in green and blue-green, and the backgrounds have red in them. The hidden numbers are in violet-red and blue. About 80 per cent of the cases see the hidden numbers and but very few see the plates as blanks. Plates 6 and 7 each have one red number on a green field, and 8 and 9 have green numbers on fields made of violet-red and yellowish dots. There are no hidden numbers on these four charts, and the color-blind characteristically fail to see anything on them. Quite regularly more fail to see the green numbers than the red ones. For the combination plates, Nos. 12 and 13, we get very clear-cut results. About 72 per cent of all cases promptly report the red 2 and 4. There were 48 who saw

TABLE 2

THE RESPONSES OF 106 COLOR-BLIND MEN ON THE VARIOUS ISHIHARA PLATES

| Plate number | Normals read | Usual color-blind read | Read correctly | | Read as usual for color blind* | | Miscellaneous readings | | Unable to see numbers | |
|---|---|---|---|---|---|---|---|---|---|---|
| | | | Frequency | Per cent | Frequency | Per cent | Frequency | Per cent | Frequency | Per cent |
| 1 | 12 | 12 | 106 | 100 | 106 | 100 | 0 | 0 | 0 | 0 |
| 2 | 8 | 3 | 55 | 52.0 | 39 | 37.0 | 9 | 8.5 | 3 | 2.8 |
| 3 | 6 | 5 | 41 | 38.7 | 52 | 49.0 | 12 | 11.3 | 1 | 0.9 |
| 4 | 5 | 2 | 10 | 9.4 | 90 | 85.0 | 4 | 3.8 | 2 | 1.9 |
| 5 | 74 | 21 | 7 | 6.6 | 83 | 78.0 | 10 | 9.4 | 6 | 5.7 |
| 6 | 2 | .. | 14 | 13.2 | 71 | 67.0 | 21† | 19.8 | 71 | 67.0 |
| 7 | 6 | .. | 22 | 20.8 | 74 | 69.8 | 10 | 9.4 | 74 | 69.8 |
| 8 | 5 | .. | 3 | 2.8 | 87 | 82.0 | 16 | 15.1 | 87 | 82.0 |
| 9 | 7 | .. | 7 | 6.6 | 86 | 81.0 | 13‡ | 12.3 | 86 | 81.0 |
| 10 | .. | (5) | | | | | | | | |
| 11 | .. | (2) | | | | | | | | |
| 12 | 26 | 2 or 6 | 14 | 13.2 | 2-77 | 72.6 | 15** | 14.2 | 13 | 12.3 |
| | | | | | 6-23 | 21.7 | | | | |
| 13 | 42 | 4 or 2 | 24 | 22.6 | 4-76 | 71.7 | | | | |
| | | | | | 2-37 | 34.8 | 5†† | 4.7 | 13 | 12.3 |

*Number of readings that correspond to the third column from the left.
†Of these, 15 read the plate as 8, which is fairly easy to confuse with 2.
‡Of these, 11 read the plate as 9 which is easily mistaken from 7.
**There were 7 others who read it as a single number not 2 or 6; 48 read it as 2 only; 9 read it as 6 only.
††There were 3 others who read it as a single number not 4 or 2; 49 read it as 4 only; 12 read it as 2 only.

only the 2 on No. 12, and 49 who saw only the 4 on No. 13, that is, 45 per cent of the whole group were quite oblivious to the violet-red numbers on these plates. Aside from these 48 above mentioned, 15 cases saw the 2 and had a hint that there was an adjoining number, reading it as something in the twenties but not 26. Seeing one number far at the side prompts a very careful search for the other number. Likewise in the case of the 42 (Plate No. 13) there were a few who could not quite make out the 2 but guessed something in the forties. For the violet-red 6 there were 9 who got only this but 23 in all who saw it, while 37 saw the violet-red 2, and 12 saw nothing else on that plate. No. 13 is found to be easier than Plate 12 although both gave 13 cases of complete failure.

If we classify the cases according to the suggestions in Dr. Ishihara's manual, the groups are as exhibited in Table 3. Here we show also the results obtained by Sibyl Terman (7). Green-blindness is more frequent than red-blindness in the ratio of about 3 to 1.

TABLE 3

CLASSIFICATION OF THE COLOR-BLIND ACCORDING TO THE ISHIHARA TEST RESULTS

| Class and condition | Miles | | Terman | |
|---|---|---|---|---|
| | Number cases | Per cent | Number cases | Per cent |
| Incompletely red-green blind | 14 | 26.9 | 23 | 21.7 |
| Nearly completely red-green blind | 5 | 9.6 | 5 | 4.7 |
| Completely green blind | 19 | 36.6 | 49 | 46.3 |
| Completely red blind | 6 | 11.5 | 8 | 7.6 |
| Completely red and about completely green blind | 4 | 7.7 | 5 | 4.7 |
| Completely green and about completely red blind | 1 | 1.9 | 1 | 0.9 |
| Completely red-green blind | 3 | 5.8 | 15 | 14.2 |
| | 52 | | 106 | |

It is obvious that the Ishihara plates are not all equally difficult; however, we may consider the results for our 106 cases of color defect from the standpoint of how much of the test they could interpret correctly. Suppose we give one point for the correct reading (as the normal eye sees it) of each plate, 2 to 9 inclusive, and one point for each single digit correctly read from Plates Nos. 12 and 13, that is, two for each of these plates. On this basis a

perfect score would be 12 points. All our cases fall below this as a condition of their being in the group. For the different point scores we then have the following distribution of cases:

| Point scores | 0, | 1, | 2, | 3, | 4, | 5, | 6, | 7, | 8, | 9, | 10, | 11, | 12 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| No. of cases | 6 | 6 | 36 | 17 | 13 | 7 | 9 | 2 | 4 | 4 | 2 | 0 | 0 |

From this it is seen that the typical case was able to read only 2 or 3 of the digits displayed. Usually this included the red 2 and 4 of Plates 12 and 13. Of the 6 cases that read no number correctly there was only 1 who claimed to see nothing on any chart beyond Plate No. 1; he appears to qualify as a complete color-blind. Some 5 per cent of those examined reported that they were able to get the digits better at the 10-foot distance than at 4 feet. The improvement was usually not very marked and it commonly applied to Plates 6 to 9.

## Summary

1) The Ishihara Test, which consists of numbers 2 inches high printed in colored dots on fields of colored dots, is recommended as a practical test of color-blindness to use in combination with routine physical examinations and by ophthalmologists.

2) This test is very serviceable in selecting cases of color-vision defect for further study.

3) In a group of 1286 university men examined by means of the Ishihara Test during 1927 and 1928, it was discovered that 106 exhibited definite color defect. This percentage, 8.2, is about double that ordinarily found with wool tests for color-blindness.

4) A group of 436 university women showed one clear case and three other faint cases of color-blindness.

5) Among the men green-blindness is more frequent than red-blindness in the ratio of about 3 to 1.

6) In a large number of the cases of color-blindness, the men are quite unaware that such a defect is present. The Ishihara Test constitutes the simplest available demonstration for convincing such cases.

7) A few cases detected by this test show better ability to read the numbers at a distance of 10 feet than at 4 feet, thus indicating greatly restricted color fields, which are practically confined to the fovea.

## References

1. [Anon.] Index catalogue of the library of the Surgeon-General's Office. Washington, Third Series, 1923, **4**, 65-68.
2. Bell, J. Color-blindness. Eugenics Laboratory Memoirs, Vol. 23. Treasury of Human Inheritance, Nettleship Memorial Volume, Vol. 2, Part 2. London: Cambridge Univ. Press, 1926. Pp. 267+41 plates.
3. Chase, A. E. Color blindness in industry. *J. Amer. Med. Asso.*, 1926, **87**, 1258-1261.

4. CLARK, J. H. The Ishihara test for color blindness. *Amer. J. Physiol. Opt.*, 1924, **5**, 269-276.
5. HAUPT, I. The Nela test for color blindness applied to school children. *J. Comp. Psychol.*, 1926, **6**, 291-302.
6. ISHIHARA, S. Tests for colour-blindness. (5th ed.) Tokyo: Kanehara. Chicago: C. H. Stoelting (selling agents for U. S. A.).
7. TERMAN, S. W. An attempt to provide a basis for grading and classifying cases of red-green color-blindness. (Unpub. thesis.) Stanford Univ. Library, 1927.
8. ————. A new classification of the red-green color blind. *Amer. J. Psychol.*, 1929, **41** (in press).

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## THE VEHICLE ILLUSION IN THE VERTICAL AXIS

There are two familiar illusion experiences which for simplicity of description we may set off by the names "vehicle illusion" and "panorama illusion." The vehicle illusion is a very common one. People frequently report and comment on it in reference to the starting of railroad trains in the station. It is always a difficult matter to be sure whether your train or the train parallel to it is the one that has started to pull out. Ordinarily when the motion is slow it is imputed to your own vehicle; you are moving and thus you account for the moving visual field. The panorama illusion appears to depend upon higher rates of speed and the hiding of the foreground or such a distance from the foreground as to make it impossible to see small details clearly. With the train or automobile rushing along smoothly at 50 miles an hour, if one gives his attention to objects 300 yards away at the side, he usually gets the panorama illusion—the objects seem to be moving by him while he is about stationary. The conditions favorable for producing the panorama illusion seem to be ideal in aviation at moderate or high altitudes.

I am not aware that any one has reported the vehicle illusion occurring in the vertical axis. The occasions when we are moved in the vertical by what we may class as vehicles are not as numerous as those providing horizontal movement. I wish to report a very compelling vehicle illusion in the vertical. This occurred at a railroad ferry. The train was broken up into half a dozen sections, which were placed parallel on the ferry boat. The boat moved across the sheet of water two miles or so in width. The adjustments made at a railroad ferry to accommodate for changes in the height of the water due to the tide have to be rather more enormous than those associated with an automobile ferry. The adjustable apron which carries the rails which are to connect with those on the ferry boat is about 100 feet long and 50 feet wide. As our boat made fast to the landing,

the edge of this apron was above the deck level some 10 feet. At first the apron was lowered rapidly, at the rate of about one foot in two seconds. The observer was standing on the platform at the end of one of the coaches. His eyes were approximately 10 feet above the level of the deck of the ferry boat. The impression was perfectly clear that the ferry boat was stationary and that the huge wooden apron was moving down to our level. When the apron reached a point about two feet above its final position, suddenly the vehicle illusion was present in a very compelling form: one experienced the boat gently rising to couple up with the rails on the apron. The movement of the apron at this time was slow and from a position 10 feet above the platform one looked out on the apron obliquely so that this great moving surface covered a large central portion of the visual field. When the rail level matched and the movement ceased, there was an after-image of movement, that is, an after-image of the vehicle illusion just experienced—the deck was sinking slowly. The observer remarked on what he had seen to the train officer who was standing by him, who said, "Yes, it always does that." The tendency, of course, is to watch the last foot or so of the movement quite intently, hence a good many people see the "funny" effect.

Brief experiences of the vehicle illusion in the vertical axis can be got from automobile and passenger elevators. Standing right at the rail of the elevator well in a garage and watching the elevator come up slowly, one gets the impression of motion just as the elevator comes to rest at the proper level. Parallel elevators with rather open lattice cages provide us a situation quite analagous to parallel trains in the station. The chief difference is that with the elevators the motion is more rapid in start and stop, and, as there is only one car rather than a succession of cars, we have much fewer opportunities for being at the same level as the adjoining car, which is, of course, the necessary condition if we are to get the illusion. The speed of elevator movement commonly used introduces a visceral-kinaesthetic factor which serves to cut short the illusion. Due to the force of gravity we are much more cognizant of having a platform drop out from under us or push up against us than we are of having it glide evenly on the horizontal axis. Our usual experience of the vehicle illusion in the horizontal occurs when we are seated, then the body is relaxed and has large areas of support which brace one another.

The vehicle illusion is not limited to the horizontal axis of motion. It can occur in the vertical if conditions are present similar to those that produce it in the horizontal. It appears to be an illusion that is quite independent of axis of motion but wholly dependent upon a certain range of speed, the involvement of a certain amount of visual field, and the absence of certain kinaesthetic factors.

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## NEON GLOW LAMPS AND THE DEMONSTRATION OF VISUAL PHENOMENA

The neon lamp is the luminant now commonly employed in the new electric signs. The rapid adoption of this type is due to several special features which it embodies. This new lamp has very low current consumption, a relatively small light output which makes it possible to be viewed directly without discomfort to the eyes, a distinctive color (or colors), and the absence of a fragile filament. These lamps may be made in the forms of thin letters or diagrams which are very legible far away or near to. Usually they are not caused to wink or flicker but are carried continuously on 60-cycle circuits, and, when looked at, the light appears to be steady. For reasons to be mentioned later, it is not necessary to flicker these lights for gaining the attention, as has commonly been done with the regular filament lamps.

A small spherical bulb lamp of this type known as Neon Glow Lamp[1] is now available at a moderate price. The bulb is 1¼ inches in diameter and is mounted on the usual screw base so that it fits the ordinary electric-lamp socket (See Figure 1). No additional resistance unit or other accessory is needed for its operation. By reason of its economical current demand and long life the lamp is admirably adapted to be used for pilot

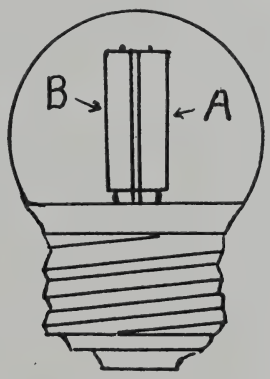


FIGURE 1

DIAGRAM OF THE NEON GLOW LAMP SHOWING THE ARRANGEMENT AND RELATIVE SIZE OF THE ELECTRODES *A* AND *B*

Each electrode is in the form of a half cylinder mounted about a central core of composition material. The electrodes flash alternately.

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[1]Manufactured by Cooper Hewitt Electric Co., Hoboken, N. J., and known as G-10, ¼ watt, 110 volt. Price, about 55 cents each. See (3) for data on this type of lamp.

purposes, serving as an indicator in any place where it is desired to show that current is being used. It is an excellent marker for showing the location of switch or valve in a dark environment. It can be used to illuminate the face of a boudoir clock or placed behind a glass plate bearing the house or street number.

Any or all of these uses apply, of course, to the laboratory, but the most unique features of the lamp for laboratory purposes is that it provides interrupted light (flashes of light) at the rate of the cycle frequency in the alternating current. I am advised by the manufacturer that the "lamp has performed when modulated at the rate of 50,000 per second." It does not come to full brilliancy until it reaches at least 100 volts. Where a readily modulated light source is desired or where a lamp capable of being lighted at high frequencies and with a minimum of lag is needed, this particular lamp deserves trial. For example, it is ideal as a stroboscopic illuminate. When used with the Victor Stroboscopic Disk (2) on a phonograph turntable without other natural or artificial lighting, it gives a very startling result in the sharpness with which the spokes on the disk stand out.

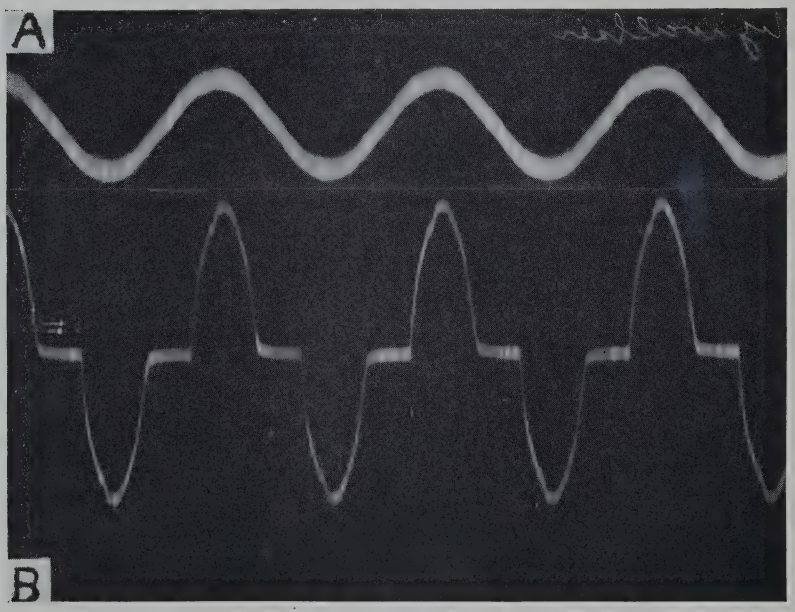


FIGURE 2

OSCILLOGRAPH TRACING SHOWING THE OPERATION OF THE NEON GLOW LAMP

*A*—the 60-cycle voltage curve. *B*—current alterations in the lamp. Current does not flow continuously but is at zero about 41 per cent of the time.

The element in this lamp is made up of two metal electrodes, each shaped like the half of a cylinder and formed around and covering a central composition core. The electrodes are about 5/8 of an inch long and 3/8 of an inch in diameter. The lamp when operated on alternating current comes to full brilliancy on one electrode, drops to zero on this electrode, then comes to full brilliancy on the second electrode. On direct current of proper voltage one electrode only burns continuously, there are no flashes. These facts for alternating current operation are clearly shown in the oscillograph tracings, Figure 2. At the top we have the 60-cycle voltage curve of the circuit on which the lamp was operated. The lower curve shows the oscillograph record for the current in the glow lamp. Deflections upward indicate the operation of one electrode while the downward deflections were associated with the illumination of the second electrode. From this tracing it is obvious that the current is not flowing continuously in the lamp. The straight horizontal lines made by the oscillograph element record moments of zero current. From zero there is a quick increase in current, much more rapid than the associated rise in voltage in the curve above. There is a rather sharp peak and a swift return to zero. The current does not return to zero as quickly as it leaves the zero line. The zero-current time intervals represent about 41 per cent of the cycle interval or of the phase interval. During zero current the lamp gives practically zero light. The neon lamp thus provides us with an interrupted light which follows the cycle rate on which it is operated. If 60-cycle current is used, each electrode gives 60 flashes per second, and the lamp as a source for lighting some object like a stroboscopic disk would give 120 flashes per second.

If one holds the Neon Glow Lamp 1½ feet or more from the eyes and fixates the electrodes, the light seems to be perfectly steady on 60-cycle current; the element glows evenly as a red-hot iron might glow. If the lamp is moved, the element gives the appearance of quivering, and well-marked lines of light appear on either side of it. If one shifts his fixation from side to side across the lamp, distinct flashes of light appear. It is this latter feature that makes the lamp a good attention-getter. One is visually exploring the road or street, fixating this and that and jerking his eyes back and forth at high speed. Suddenly in the periphery of vision or perhaps reaching right across the fovea one sees a line of transient flashes all about the same size and shape but of decreasing brilliance. The effect is challenging. There is nothing else quite like it. A brilliant twinkling star provides the same effect, but the flashes from this are so faint that most people have never noticed them.[2] The strung-out flashes from the neon

[2]The star flashes are particularly easy to see if one looks at some bright star through a 6-power binocular and at the same time gives the glass an oscillation or circular motion. If the eyes are moved back and forth across a street arc light, we may readily observe whether the lamp is operating on A.C. or D.C.

sign are so large in area and so brilliant that the eyes naturally turn to their source. Here they see outlined in a steady glowing light some word or diagram, and the conditions are most favorable for perception. The interrupted light of these lamps plus the factor of eye movements plus the factor of persistence of retinal impression is the formula which probably provides a better attention-getting and perception-promoting result than came from the earlier flash sign.

The Neon Glow Lamp is an admirable device for demonstrating the phenomena of persistence of vision (modern form of the firebrand experiment) in the lecture room or laboratory. Moving the lamp rapidly back and forth or twirling it through a circle in a dark room, or even in a well-lighted lecture room, provides a visual stimulus of striking quality that can be seen by everyone. Due to the arrangement of the electrodes and to the characteristics inherent in the operation of this lamp as shown in the oscillograph tracing, different kinds of spots can be secured according to the angle from which the eye views the lamp. If the lamp is held so that the observer sees only one electrode and is then moved laterally back and forth, spots of light $\frac{5}{8}$ of an inch high by a width according to the rate of movement will appear. If the movement is rapid, the flashes will be long and the gaps long. The latter have somewhat greater width. The observer is seeing only 60 flashes of light per second, that is, the flashing of one electrode. If the room is dark, a phantom or dimly lighted spot will appear midway between the main flashes. This is due to the illumination of the electrode which is now on the back, away from the observer. The light from this electrode reflects against the glass at the rear of the lamp. This phenomenon of the phantom dashing in between the bright dashes will be seen only against a dark background. Diagrammatically and only approximately, Figure 2 represents the varieties of flashes, for convenience of illustration showing these flashes as dark spots on a white background. The flashes described above are illustrated in *A* of Figure 3. Now if one turns the lamp so as to view the upper end of the two electrodes, i. e., with one electrode above and the other below, as in *B* of Figure 3, lateral movement of the lamp in the direction of the arrow gives flashes as a series of curves, as illustrated. If the lamp is rapidly moved back and forth through an 8-inch arc, one can secure the effect of standing luminous waves that show rather short gaps halfway between the trough and crest of each ascending and descending curve. Slower movement produces crossing or interlacing of the approximate semicircles of light. If the lamp is turned from this position 90° so that the end of one electrode is seen at the left and the other at the right, as seen in *C*, Figure 3, then rapid movement shows pairs of crescents back to back with fairly large gaps between pairs. The inequality in the spacing is caused by the combination of the flashing time of the electrodes and the angular rate of movement of the lamp. If the electrodes are viewed from the side so that each one is seen equally,

*D* of Figure 3, rapid movement gives pairs of spots each half as wide as shown in *A*, and in arrangement as would be expected from a lateral view of *C* in our figure. With these various possible combinations the flashes are quite interesting objects for study, and if the lamp is mounted on a mechanical device to rotate it or oscillate it, very definite effects can be predicted. Used in this manner it provides us an apparatus for experiments in observing transient visual phenomena.

The Neon Glow Lamp is proving of much service in our Laboratory as a means of demonstrating and making preliminary study of eye movements. As everyone knows, it is difficult to control the visual fixation of an observer. With the very best cooperation, the observer may be unable to report whether his fixation has been unwavering just prior to and at the moment of a certain exposure. The neon lamp is of service in this connection. Placed behind a screen with a very small opening, it can be used as a

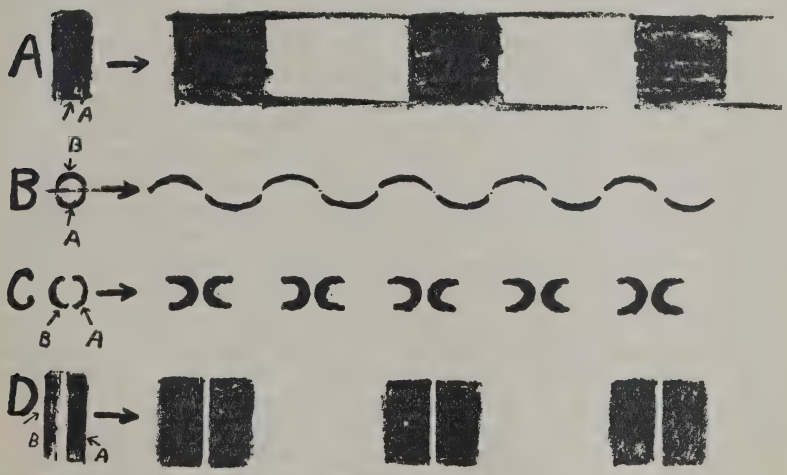


FIGURE 3

PATTERNS OF THE FLASHES SEEN BY THE EYE WHEN THE NEON LAMP IS MOVED RAPIDLY

Small *A* and *B* designate the two electrodes, and the flashes are shown as dark spots.

*A*—viewing only one electrode. The flashes as diagrammed beyond the arrow are 60 per second. *B*—electrodes viewed on end, one above the other. Movement of the lamp gives the appearance of standing waves. *C*—two electrodes viewed on end, one at the right and one at the left. Movement gives pairs of crescents back to back. *D*—the two electrodes *A* and *B*, both in side view. Movement gives pairs of dots. If the lamp is still and the eye is moved, similar patterns of dots will be seen but they will be farther apart due to the high speed of angular rotation of the eye.

comfortable fixation point. If fixation wavers, the unique phenomenon of the flashes appears. Wavering fixation is thus more likely to be reported than when the fixation point is an ordinary steady light.

In studying the visual phenomena associated with winking the eye or alternately opening and closing the eyes (1), the flashes aid in describing the observations, by reason of the fact that they appear as a new feature which comes in association with eye movement. Subjects can also make an estimate of how many spots of light were seen trailing off and thus state the speed of their eye movements.

For directly demonstrating the high speed characteristics of saccadic eye movements and the interplay of these with pursuit movements and with compensatory movements, I know of nothing that at all approaches the neon lamp in usefulness. With the lamp mounted at a distance and with fixation points placed one on either side and a pair arranged in the vertical representing an equal visual angle, the subject is instructed to look back and forth or up and down between the fixation points giving attention to the flashes and making an effort to count them. From the way the flashes are stretched out he has a definite impression of the speed and notices that this is quite regular for either the horizontal or the vertical movements, but is slower for the vertical. He can make an estimate of the relative speed of different angles of movement by changing the positions of the fixation points. The results are only approximate but much better than can be obtained from observing the eye movements of someone else. In fact they are only second to photographic records. They supersede these in one respect, which is that the subject is also observer and has his results immediately in combination with the experience rather than waiting to see them on the tardily developed photographic plate.

If the subject observes the lamp at a distance, fixates it carefully, and then turns his head slowly, he sees no flashes or flutter of the element. More rapid movements of the head or body cause flashes and prove that his compensatory eye movements are not quite accurate or adequate. If the observer fixates the lamp while it is moved mechanically or in the hand of someone else, and if he holds his head still, his eyes will be executing pursuit movements. If the movement of the lamp is slow and regular, it will appear only as a glowing iron entirely without flashes. When the movement is more rapid or jerky, flashes will appear, proving that the pursuit movements have become inadequate for following the stimulus and that they are being supplemented by saccadic movements. The limiting rate of movement in a stimulus which can be adequately matched by pursuit movements of the eyes can be approximately determined without difficulty. If, in place of moving the lamp, it is mounted in a stationary position and the subject holds a pencil at arm's length in line with the lamp and fixates the pencil, while he moves it slowly back and forth, he will see a great number

of fairly compact flashes from the lamp. In this way he can demonstrate to himself that the pursuit movements are very much slower than the saccadic movements.


## Summary

For advertising purposes and as an adjunct for laboratory experiments the neon lamp has the following advantages:

1) It provides flashes of light at a rate governed by the cycle frequency of the current used. It will respond to rates as high as 50,000 per second and is nearly instantaneous in its rise to complete brilliancy and in its decline to zero.

2) Due to the persistence of vision, flashes of light appear when the lamp is moved or when the eyes are moved. These flashes are very obvious and challenging as a new and unexpected feature in the environment, therefore they are quite attention-compelling. It is thus unnecessary to "flicker" the street sign made from this type of lamp.

3) Neon light is not too strong for direct vision. A small electrode of this character may be visually fixated without discomfort to the eyes.

4) Used as a control to insure steady fixation in visual experiments, this lamp aids the observer in reporting his fixation failures.

5) By arranging fixation points around a stationary neon lamp the approximate rate and directness of saccadic eye movements can be observed (counting flashes) by alternating fixation between the points and thus examining horizontal and vertical axes of movement.

6) The adequacy of pursuit movements in reference to the speed of a target (in this case the lamp) can be observed. If the pursuit movement has to be supplemented by a saccadic movement, flashes appear. Similarly, compensatory eye movements associated with head movements and body movements can be studied.


## References


1. Carr, H. A visual illusion of motion during eye closure. *Psychol. Rev. Monog. Suppl.*, 1906, **7**, No. 31. Pp. v+127.
2. Miles, W. The Victor Stroboscopic Disk for visual experiments. *Amer. J. Psychol.*, 1928, **40**, 312-313.
3. Moore, D. M. Recent developments of Moore gaseous conductor lamps. *Trans. Illum. Eng. Soc.*, 1926, **21**, No. 2, 176-185.



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## THE NEON LAMP AS AN AID IN STARTING SYNCHRONOUS MOTORS AND AS A SPEED INDICATOR

Inasmuch as a synchronous motor will run at only a certain speed or a multiple thereof on alternating current of a given frequency, it is necessary that its armature be revolved at this exact speed by outside force before the machine will function as a motor. Because of the difficulty in judging the exact speed at which to turn the armature, it is often a trying task to start this type of motor on laboratory instruments such as the Dunlap Chronoscope, the Lisston Tonoscope, and other apparatus embodying this source of constant speed power. With the aid of a Neon Glow Lamp, such as described in this number by Miles, the task becomes relatively easy.

The minimal speed of any synchronous motor is given by the simple formula:

$$\text{rev. per sec.} = \frac{f}{N}$$

in which $f$ is the frequency of the current supply in cycles per second and $N$ is the number of poles in the magnetic field. Thus, the ordinary speed of a 12-pole motor when supplied by 60-cycle current is five revolutions per second or 300 per minute.

If a circular ring composed of 12 equally spaced black segments, on a white background, is fastened to the rotor or the drive shaft of the motor, each of the 12 segments will pass a given point five times per second. In other words, any one point on the revolving ring would actually be alternately white and black 60 times per second, although it would appear gray due to the rapid mixture of the colors.

If, however, the revolving ring is illuminated by a neon lamp which is supplied by the same current and which is also alternately light and dark 60 times per second, the segmented ring will seem to be standing still. This illusion is, of course, due to the fact that the ring is lighted only while the black segments are in a given position and is dark during the time that it takes them to move to the next position of the adjacent black segment. Because of the very short time interval between the successive appearances of a black segment, positive after-images fill in the gap, and the black appears to be black all of the time.

In starting the motor, it is simply necessary to spin the rotor until the ring appears stationary, at which time the normal speed will be reached and the motor will continue to run on its own power. Such a device may be utilized on any small synchronous motor, since the number of segments needed in the ring is:

$$\text{no. of segments} = \frac{f}{\text{rev. per sec.}}$$

and since

$$\text{rev. per sec.} = \frac{f}{N}$$

it will be seen that

$$\text{no. of segments} = N$$

It will be necessary then to make the same number of black segments on the ring as there are magnetic poles in the motor. As long as both the neon lamp and the motor are operated on the same current supply, the scheme will work equally well with any frequency current. It will readily be seen that the ring would also appear stationary at any multiple of the motor's minimal speed, so the device is convenient and useful in starting the motor at some multiple of its slow speed.

In other instances this same device can be utilized as a speed indicator. Thus, a ring with 12 black segments illuminated by a neon lamp on a 60-cycle current would look to be stationary at 300, 600, 900, 1200, etc., r.p.m. By counting the number of times that the static appearance occurs while the motor is coming up to speed, its approximate speed can be estimated. A slight deviation from one of the above speeds will be indicated by an apparent slow revolution of the disk to the right or left. Using these same principles, it is possible to divide the ring so that it will appear to be stationary at any desired speed. The number of segments necessary is obtained by dividing the desired speed by the frequency of the current available. This makes a very inexpensive and yet an extremely reliable instrument for adjusting a piece of apparatus to a desired speed.

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## HIGH-SPEED LAMPS FOR TACHISTOSCOPIC WORK

The development of "high-speed" incandescent lamps provides an important piece of apparatus for use in tachistoscopic and related experiments. Control of exposure by the simple method of switching on and off the current supply of an incandescent lamp has heretofore been seldom resorted to, for the obvious reason that in brief exposures the time taken by the ordinary lamps in heating and cooling is objectionably great in relation to the exposure times themselves. It is plain that in the control of exposure in tachistoscopic work one should have control of the rates of appearance and disappearance of the exposure field; indeed in the interest of simplicity these rates should be kept high in relation to the rates of response of the retina. In short exposures the visual response must be regarded not as a function simply of time, but of the total energy acting on the retina, which is an integration of the light intensity throughout the interval.

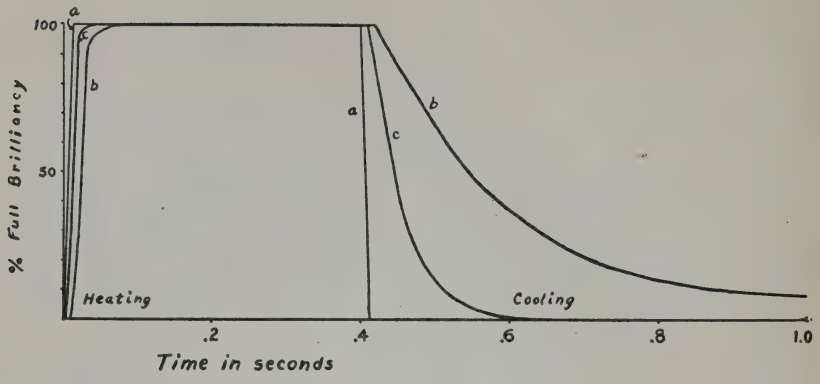


FIGURE 1

*a*, falling-screen tachistoscope; *b*, 100-watt regular stereopticon lamp; *c*, 150-watt high-speed lamp

The ideal type of exposure would be one in which the illumination of the field was changed instantaneously from minimal to maximal, and conversely; but it is obvious that this ideal condition can only approximately be realized in practice. A close approach can be made by passing before a focussed beam a narrow slit in a very rapidly moving screen. Under best conditions the curve of the illumination of a field opposite the beam will approach a rectangular form when plotted as a function of time, i.e., the curve would be rectangular if plotted in Figure 1. The usual forms of apparatus which utilize this method do not usually, however, obtain curves as desirable as this; they approach more nearly the form of that shown as *a* in Figure 1, which was given by a falling-screen tachistoscope used by the writer (1, p. 200). But, fortunately, because of the inertia properties of the retina just mentioned, one does not need to attain ideal conditions, and an exposure like that shown as *a* in Figure 1 is entirely satisfactory.

The familiar types of incandescent lamp give curves of the kind shown as *b* in Figure 1, and are slow enough in their operation to introduce considerable error into measurements of exposure. Certain lamps of recent development, on the other hand, compare favorably with curve *a*; these are the lamps designated as "high-speed" lamps, one of which is shown in curve *c*, Figure 1. It is plain that this type of lamp recommends itself for use in tachistoscopic work.

Under the designation of "high-speed" lamp several varieties may be included. These lamps have been developed and their properties studied in the research laboratories of the General Electric Company, primarily in response to an expression from the U. S. Signal Corps of the need for a

rapid-acting lamp in war signalling (2). One of the most rapid lamps developed is one which differs from the ordinary lamps in several respects, but chiefly in that the filament is of tungsten drawn particularly fine, immersed in an atmosphere of hydrogen. The use of a ribbon instead of a wire filament gives greater speed, but the vaporization rate of the ribbon is relatively high, and hence the life of the lamp is short. Immersion in argon gives longer life and greater efficiency, but at the expense of an appreciable reduction in speed. It is apparent that the choice of a lamp for a given purpose will depend not merely on its speed characteristics, but will consider also such factors as sturdiness, life, efficiency, and initial cost. For psychological work the wire-filament, hydrogen-filled lamp will probably be the most satisfactory. This lamp may be obtained in several sizes. The writer has had most experience with the 150-watt, 110-volt, G-25 bulb type, which is quite satisfactory in performance and life. The 250-watt, 110-volt, G-30 bulb type is also recommended.

The 150-watt lamp heats up to full brilliancy in 0.022 sec., and cools off to the point where the filament barely glows in 0.280 sec. The corresponding figures for a 100-watt regular stereopticon lamp are 0.068 sec. for heating and 0.786 sec. for cooling. A 400-watt, hydrogen-filled lamp heats up in 0.069 sec., and cools off in 0.834 sec. The 400-watt stereopticon lamp takes 0.196 sec. to heat up, and 3.914 sec. to cool.[1] The advantage of speed is clearly in favor of the hydrogen-filled lamp, by a factor of 3 or 4, for both heating and cooling.

In ordinary tachistoscopic work the effective speeds of the hydrogen-filled lamps are even greater than the above figures might lead one to believe. The reason is that much of the time included in the figures is not significant in respect to a given exposure. A reference to Figure 1 will help to make this point clear. The curves show that a considerable portion of the total time is expended (1) in heating up the filament to the point at which the light emitted is sufficient to illuminate a field, (2) in bringing the heating curve through the upper portion of its course, from about 90% to full brilliancy, which because of Weber's law is not a significant change, (3) in the initial drop of the cooling curve to the point where the diminution of light becomes significant, and (4) in the last stage of the cooling, extending beyond the point at which illumination of the exposure field has practically ceased, and the filament is approaching room temperature. This means that the middle—and fairly straight—portions of the curves are the really significant portions to consider in most exposure work. In other words, the "reduced" times, i.e., the times involved in heating (or cooling) from about 20% to 80 or 90% of full

[1]Data kindly supplied by the Engineering Department of the Edison Lamp Works.

brilliancy, are the significant values for a designation of speed in this connection.

Unfortunately, we do not have sufficient data to say what the "reduced" times will be for the lamps mentioned. Some data are available, however, on similar lamps of lower voltage (2), and a rough calculation shows that for them the "reduced" times of heating are but 25 or 30% of the total times, and for cooling are 20% or even less. Indeed, the effective cooling time is about the same as the heating time. Probably these values will be corresponding for the higher voltage lamps.

It should be noted that the "high-speed" lamps are not made up as a commercial article, but are manufactured on order. Their cost, however, has in the writer's experience been but slightly in advance of the commercial lamps of equivalent rating.

A word of caution should be added regarding the use of a hydrogen-filled lamp. In some instances a leak may develop at the seal, which allows enough oxygen to enter to cause an explosion when the lamp is lighted. For this reason the lamp should be enclosed in a housing of some kind, or covered with a fine wire netting, to afford adequate protection from flying glass. Care should also be taken to provide proper ventilation, so that the heat may be conducted away, or else the lamp will not perform at normal speed, and its life will be reduced. Aside from these precautions the "high-speed" lamp is operated like any other incandescent lamp.

References

1. Wever, E. G. Figure and ground in the visual perception of form. *Amer. J. Psychol.*, 1927, **38**, 194-226.
2. Worthing, A. G. Flashing speeds of incandescent signal lamps. *J. Franklin Instit.*, 1921, **191**, 231-257.

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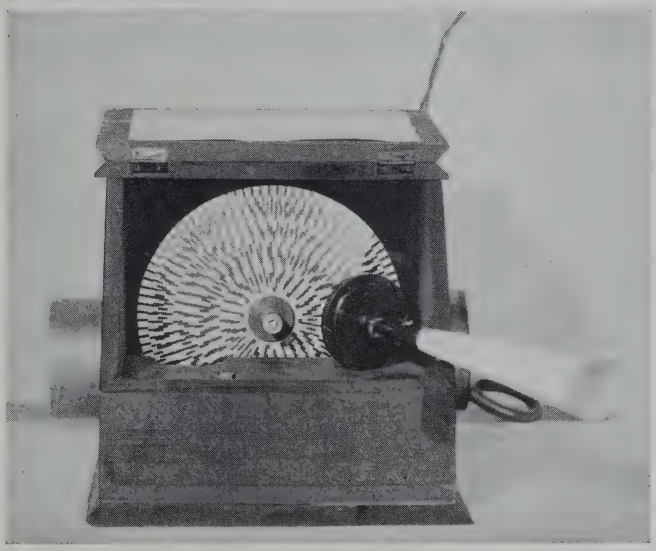
## TWO INDEPENDENT DEVELOPMENTS OF THE STROBILION

On the subject of culture, anthropologists have been considering at least two rival theories. One, the Rivers-Elliot-Smith "heliolithic culture" hypothesis, holds that much, if not most, culture has spread directly or indirectly from one source. Extreme proponents of this view hold that discoveries or developments of specific culture items are in general made by a single person, and so rarely more than once. The rival theory states that discoveries, independently made by more than one person, have occurred frequently in different portions of the world. A compromise position between these two views is becoming increasingly popular. It may perhaps be stated as follows: cultures do spread, but are frequently added to in a

similar manner by a number of people. It is the purpose of this note to show how two researchers have built similar structures on the same foundation.

A device which made use of manometric flame and stroboscopic principles for the visual demonstration of vocal sound effects was developed by E. W. Scripture (2), then of Yale. This was altered by C. E. Seashore (5) and C. F. Lorenz (1) and put into more general use by Seashore under the name of "tonoscope" (4).

Scripture has recently constructed a new form of this apparatus, which he terms the "strobilion" (3). From the description and photograph it



seems to be practically identical in its essential details with one developed at the Stanford laboratory. The accompanying photograph is of the Stanford model, which is called the "true tone analyzer."

In the autumn of 1926, Mr. M. A. Lissman, a graduate student in the Department of Electrical Engineering at Stanford, was an auditor in the writer's class in the psychology of music. At this time he became acquainted with the early model of the Seashore tonoscope. During the following winter months he constructed the Stanford model. To the writer's knowledge no mention of this has appeared in the press except for a "doctored" account in a San Francisco newspaper.

The Lissman instrument with motor and gas tank attached weighs but 19 pounds, so is convenient for demonstrational work. The synchronous

motor runs at two speeds—for the optimum display of the voices of the two sexes. A worthwhile improvement over the older instruments is the addition of a wire which is wrapped around the burning point. While the instrument is in operation, the wire becomes hot, and relights the gas whenever too great a voice pressure makes this necessary. While the instrument has not had much use as yet in voice training, it conceivably possesses possibilities in this connection.

### References

1. Lorenz, C. F. A stroboscopic frequency-meter. *Elect. Rev. & West. Electrician,* May 17, 1913.
2. Scripture, E. W. Elementary course in psychological measurements: Exercise 16. *Stud. Yale Psychol. Lab.,* 1896, **4**, 135-137.
3. ————. Das Strobilion: ein Apparet zur Sichtbarmachung der Tonhöhe der Stimme. *Zsch. f. Psychol.,* 1928, Abt. 2, **59**, 166-170.
4. Seashore, C. E. The tonoscope. *Psychol. Monog.,* 1914, **16**, No. 69, 1-12.
5. ————. A voice tonoscope. *Univ. Iowa. Stud. Psychol.,* 1902, **3**, 18-29.

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# BOOKS

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EPSTEIN, A. L. *Son i Ego Rasstroistvo.* (*Sleep and Its Disturbances.*) Leningrad, Moscow: Gosudarstvennoi Izdatelstvo, 1928. Pp. 408. 3 rbs.

Until recently the pathology of sleep was restricted to insomnia and partly to hypersomnia. The pathology of sleep as the teaching about disturbances of a precise function with all their typical clinical characteristics did not yet exist—hence the deficiency of theoretical knowledge concerning the physiology, mechanism, and structure of sleep. However, the data of the last epidemic of lethargic encephalitis led to the finding of those physiological and pathological grounds which made it possible to unite various sleep disturbances in a harmonious organized system of sleep pathology. At the same time there was also elucidated the question concerning the mechanisms of normal physiological sleep, as of a function complicatedly coordinated and complicatedly centralized in the brain. This book by Dr. Epstein aims to give a systematic exposition of present day theories of sleep and of its disturbances. The fact that the author pays much attention to the mechanisms of normal sleep makes the book of great interest from the standpoint of general psychology.

In the first chapter the author gives a general conception of the anatomico-physiological basis of normal sleep, according to different data of the literature on the question. An attempt is made to establish a correlation between the anatomical data and the West European physiological theories of subcortical mechanisms of sleep, on the one hand, and Pavlov's teaching about the cortical mechanisms of sleep, on the other hand. After this anatomico-physiological introduction, an exposition of somatic and psychical alterations during sleep follows (Chap. 2). This chapter is based not only upon the data of the literature but also upon the personal investigations of the author. Out of the somatic modifications, the author annotates, in the first place, the relatively small group of visceral altera-

tions; this done, he gives a detailed description of neuro-muscular alterations. The analysis of psychical modifications during sleep (the most detailed description is given for the cases of "hypnic attention," "hypnagogic hallucinations," and of "ascending psychomotorness") is of special interest because the author tries to correlate these modifications with the clinical symptoms of the disease (autism, loss of the feeling of reality, hallucinations, double-personality, physical influence, delusion, etc.).

Chapter 3 offers a classification and a general semeiotics of sleep disturbances. The next six chapters contain a description of separate groups of these disturbances: casual, secondary, constitutionally neuropathic, reactive, symptomatic, and essential. (The greatest attention is paid to the third group.) In Chapter 10 ("Sleep and Psychoses") the author gives a historical analysis of different conceptions of the correlation of sleep and psychoses, and states the following three stages: of analogies, of clinical observations, and of pathophysiological and pathopsychological analysis. In Chapter 11 we come upon the pathological states related to sleep. Finally, in the last chapter the author speaks about sleep disturbances from the standpoint of forensic medicine. The book is written very concisely, almost in the form of a conspectus, and offers a summary of all present-day theories of mechanisms of sleep and of sleep pathology; its appearance is, therefore, of great value.

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Gaskell, A. *What Is Life?* (Introductions by K. T. Compton and R. Pearl.) Springfield, Ill.: Thomas, 1928. Pp. 324.

One view of science, regarding science for the moment as merely a more or less agreeable means of passing the time, holds that the business of science is transacted through the discussion of theories. Unfortunately, this conception is half correct. But everyone is familiar with those amusing states of mind (usually evidenced by other people) in which the wonders of nature assume significance solely in relation to "my" theory. Hence the danger of taking theories too seriously; one's personal theory especially—because it may turn about and bite one. This general notion as to what science is about sometimes results, moreover, in the tacit assumption that the building of theories is exclusively a literary performance. Most books about biological theory have been of this character. It is relatively easy to speculate in print about animals and plants, because (so far as concerns a number of matters of prime importance) it may be difficult for anyone really to prove that you are wrong. And in this case it becomes the duty (?) of science to prove whether one is right.

A good many years ago it was suggested that the substance of living cells constitutes a special kind of molecule—perhaps all the substance of one cell might be such a molecule. In spite of the large molecular weight now assignable to some proteins as they exist, for example, in solution in a blood, there is good reason to distrust the "giant molecule" conception of protoplasm. The chemical events occurring in it are altogether too much like those familiar in non-living systems. Little or nothing has been gained, save obfuscation, by regarding protoplasm as ineluctably "colloidal," with the implication that its phenomena do not make sense in terms of classical physical chemistry, because "colloidal" must itself be so explained. At the opposite extreme, it has appealed to a number of writers, notably physicists and physical chemists, that perhaps the peculiar cussedness of protoplasm, regarded as an engine, might lie in its escape from the restrictions of the second law of thermodynamics. The rules according to which the thermodynamics game is played are derived from observational acquaintance with matter in bulk. If protoplasm is essentially a series of minute reaction chambers, each or many of them perhaps including but a few molecules (of some particular sort), then the statistics of energy distribution might permit, with small enough numbers, very special and peculiar deviations from the ordinary expectation of entropy increase. This point has been made repeatedly, and apparently independently, by such men as Gouy, Freundlich, Donnan, Lewis, and most recently by Page. Like most such things, it is doubtless contained also in Gibbs's celebrated memoir. But the trouble is that until someone demonstrates that organisms *do* evade the Second Law, one need not get excited about it. Where the chemical activities of living matter are most directly open to inspection, as in the metabolism of the chemo-synthetic bacteria or in muscle, we find no reason to postulate rules other than those adequate for non-living systems.

Again, it was at one time suggested that enzymes represented the stuff of life, the essential physical substratum of liveness. This was safe enough, because no one knew just what enzymes might be. When chemical analyses of protoplasm were first made in a comprehensive way, no one substance could be picked out as the "live" component of protoplasm; but the enzymes had not at that time been recognized as separable constituents of protoplasm, and this difficulty was not overcome until Buchner's work with zymase. With increasing work devoted to the mechanics of enzymatic catalysis this sort of mystification has lost some of its punch, although in a different, chemical sense the enzymes of the hydrolytic type do in a way bridge the gap between the living and the not-living.

Throughout all this development there has run the attempt to find, in one way or another, *a* form of matter which might be held responsible for livingness. Its most recent aspect has been the thought of recognizing in the gene of the mendelian analysis a "unit of colloidal living substance."

There is reason, however, on which to regard the exposition of this view as perhaps deficient, since there arise logical problems associated with the notion of a gene which seem to interfere with speaking of a gene as a substance. The conception of proto-protoplasm as *engine,* rather than as "*a* substance," still awaits development. The essence of this idea is that livingness depends on structural arrangement of parts rather than on the presence of matter of a unique type.

The author of the book under review courageously attacks the question, "What is Life?" by way of the substance concept, but with a new approach, one verbally at least more in tune with the newer physics.

Assuming a condition of critical concentration of ions, and the (modified) Bohr atom, the author states (pp. 113-157) a theory of affairs in living matter, which I venture to condense to the following.

There is postulated for the origin of life a local, momentary condition of ionization in which the crowding of ions is such that some atomic domains (outermost electronic orbits) overlap. This is assumed to acquire critical attributes when there arises proximity of some atom or ion which will readily part with an electron to hydrogen ions, preferably to a $H_2^+$ ion (positively charged hydrogen molecules having two nuclei and one negative electron are known to be capable of a short existence). When the negative electron of the negative atom or ion gets into the field of the positive hydrogen nucleus of the $H_2^+$ ion with *its orbital* electron at aphelion, *"something is bound to happen"* (italics of the original); one possibility is that the onrushing negative orbital electron will directly collide with the positive electron and will form an exceedingly close union with it. Such a union would be unlike a hydrogen atom, because a unit of the postulated sort would not have the orbital electron revolving at a distance from the positive electron. It could not form [at least the usual] chemical combinations. And it could not become a "charge" on another atom. But it may itself capture electrons and build up a new structure (unless it be torn apart by the fields of other atoms), with electromagnetic properties peculiar to itself. (One is reminded that of the lighter elements, making up the greater part of protoplasm, the possibility exists that isotopic segregations have occurred. The single attempt known to the writer to have been made to test this, with reference to chlorine, gave a negative answer.)

From this hypothetical (but, according to Professor Compton, not impossible) structure, there is deduced the existence of a "dual system"—one part material, the *Y*-system, since built of atoms; the other immaterial, the *Z*-system, since "not patterned after the manner of the chemical elements." The dual system describes certain properties of organisms; the *Z*-system accounts for the living state and answers to life—"*is* the life of the organism."

On this basis, life is an immaterial quantity; death is the separation of

*Y* from *Z*. From the peculiar manner of combination of the *Y* and *Z* components, the psychic properties of life emerge.

In just what way such a scheme really differs from, for example, the nomological system of Mach, which happened to employ different letters, it is hard to see. Mystical notions are not improved, fundamentally, by draping them in physical terminology. To speak of a type of electron plus proton combination hitherto unrecognized as "immaterial" because it does not fall into the types hitherto recognized in the category of matter, is a rather weak sort of trick. Its importance in this book is due to the fact that having once used the word "immaterial," the author associates with it all the "non-material" attributes of organisms about which she has read. Since life is defined (*Z*) as a quantity, "it follows that the quantity 'life' is identical with 'soul,' and all problems of the soul and of mind are problems of the quantity 'life.' "

This theory, developed as an exercise in logic, is preceded by a discussion (pp. 1-112) of the nature of theories in general, of organisms, and of matter and the atom. It is followed by an examination of the elements of originality contained in the theory. Among these (pp. 169 *et seq.*) are several concerned with psychology. The method of approach is advanced as a new, exact, quantitative method, to be based upon such assertions of the theory (though not, as the reviewer sees it, necessarily derived from it; at least the derivation is not made clear) as the following.

*Mind* is the electrical organ (*Z*-system) within the brain. The method of investigating it will have to be the exact methods of the physicist and mathematician. We are not told how they are to be applied. However, it is pointed out, for example, that the superior psychic powers of adult men indicate that the human brain carries a far greater and more complex *Z*-system than the ape's brain carries, the former being acquired during prolonged infancy. On this basis, the duration of infancy, it is proposed to measure the comparative intelligence of races. How infancy is to be measured is not indicated.

There is also given (inevitably) a theory of the cause of cancer and a theory of heredity and a consideration of the origin of species, also a glossary of scientific terms, which is well done.

The proof of this notion is to be had only by physical examination of living matter. The conditions of abiogenesis, the construction, that is, of conditions under which *Z*-systems might form, are not described. The emission of radiation on death might be looked for, but well-known investigations show that "living" and dead albumin do not detectably differ in energy content; Lepeschkin's recent work is of dubious significance in this connection. What use the author might have made of Gurwitsch and his "mitogenetic rays" one is curious to learn. As Pearl points out in his introduction to the volume, the speculation is original and ingenious, though

the biological deductions given as derived from it are unquestionably premature, based upon a wide and varied reading not always critically sifted, so that the argument is sometimes touchingly verbal. It seems to be felt that its central theme may help to overcome what Pearl speaks of as the "quasi-religious inhibition of efforts at investigation of the transition zone between non-living and living matter"; that there is such inhibition one is permitted to doubt. Experimental abiogenesis can scarcely be undertaken without knowing more than we appear to know at present about the essential features of what one would set out to create. That the extension of physics and chemistry to the realm of the living will result in the amplification of conceptions of "matter" seems reasonable; that Mrs. Gaskell's mode of association of protons and electrons may represent the sort of thing which can occur is perhaps valid; but at present the reasoning which this book holds out to support such a conception seems vague and somewhat unnecessary.

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Mason, F. [Ed.] *Creation by Evolution.* New York: Macmillan, 1928. Pp. xxii+392. $5.00.

An admirable attempt has been made by an intelligent editor to gather into one volume a plain statement of those aspects of the evolution concept which seem susceptible to simple treatment. The collection is introduced by Sherrington, following a somewhat unnecessary foreword by Henry Fairfield Osborn. An air of authority is given by the historical statements under the names of the contributors in the table of contents, which indicate the present academic status, connections, and achievements of the writers of the several chapters. (Curiously enough, there is a misprint in the title of the chapter due to MacBride.) In orderly succession the presentation ranges in 24 steps from considerations of the nature and meaning of "evolution" (Jordan, Thompson); its reality as a process (Jennings); the types of evidence requiring its recognition (Parker, MacBride, Conklin, Scott, Bather, Gregory); the evolutionary panorama (Woodward) of plants (Gager, Berry), butterflies (Poulton), bees (Shipley), ants (Wheeler), horses and elephants (Loomis), birds (Watson); the lineage of man (Lull, Gregory, Holmes, Smith); the inevitable notion of progress (Huxley); the problem of mind (Morgan); and a schoolmasterly summary of the evidence for evolution (Newman). The chapters close with a list of recommended books; the editor has supplied as tailpieces a variety of suggestive quotations from diverse sources; and there is a useful index.

Probably, if the truth were told, biologists are somewhat bored with

evolution. As a conception, it is necessary; as a means of codifying known facts about organisms as an assemblage, it is convenient; it reconciles what has been learned about the diversity of organisms. As to "proving" it, this is scarcely in question. To borrow a phrase from J. W. N. Sullivan, our objection to alternative views is not that they are "wrong," but that "they look so funny." The phrase "experimental evolution" once had power, but organic change of heritable character induced by experimental treatment is not very much to the point. One cannot "experiment" with 100,000 years. So that adherence to the doctrine of evolution becomes a matter of acceptance by consent rather than of "experimental proof." When one considers the boldness of speculation formerly rife as to the "causes of evolution," one is at least struck with admiration for the youthful vigor of biology which permitted or even required such a froth, because sober reflection might well suggest that, until something more has been learned about the actual nature of living matter, such speculation may be an agreeable means of passing the time or of cultivating rhetoric but can be fruitful only in some dubiously accidental way.

One notices with gratitude that in the main the contributions to this volume have been content to describe phenomena, within their direct experience, which require for their rational comprehension a relationship of organisms through community of descent with progressive change. Since the book is designed to combat unintelligent, emotional antagonism to evolution, this is obviously the most decent and probably the most effective method. These are not then so much sermons about evolution as recitals of fascinating things seen.

A reservation must perhaps be made in connection with the treacherous term "progress." Julian Huxley considers that the course of adaptation, as made out paleontologically and by the inspection of living forms, requires that we speak of *biological advance* as real *biological progress*. It is not clear that the notions of temporal *advance* and of unfolding complexity have here been sufficiently disassociated. One related difficulty is thought to be disposed of by Huxley's view that the coincidence of his "biological progress" with much of our own common conception of progress and value is due to the fact that man happens to be himself in the main stream of biological progress, or at least likes to think so. Doubt may well be raised as to the extent to which this solution of the problem of teleology can be satisfying or is just superficially clever in a verbal way. Huxley conceives that perhaps "the forces of evolution conspire to act as 'a power, not ourselves, which makes for righteousness.'" Heredity, incidentally, is here referred to as a "force," which is neither illuminating nor correct.

As Gregory puts it in this volume, the knowledge that man has "struggled upwards to his present estate from less intelligent animals is still practically denied to the majority of mankind." Biologists have not infrequently

sought to oppose smothering dogma by preaching the "gospel of evolution." Of course, it is not so important after all that the majority of mankind is uninstructed in biology, but the fact that they may be dogmatically denied the privilege is very important indeed. The tone of the present book is in this respect good, and its effect should be lasting. The chapter dealing with apes (S. J. Holmes), though in places "soft" as to interpretation, tells an interesting story of their human, not to say humane, side. The section by G. Eliot Smith tells us in general terms, and with freedom from controversy, the results obtained from the comparative investigations of localization within the brain, and of the closeness of the similarity of that of man and of chimpanzee and gorilla. He concludes that both brain *and* mind of man are the results of evolutional development. Detailed evidence for this, in terms of behavior, is of course not provided.

A delightfully written chapter is that by C. Lloyd Morgan on "Mind in Evolution." He sees in all cases to which the word evolution can be applied the general fact of *passage,* according to a plan which can be formulated (after the event), "things" being clusters of events in passage. Events pass, but a *state* of flowing events may be persistent, as with social organization or with mind. Evolution he would use in a restricted sense to denote progressive upward change, implying the directive immanence of God; dissolution, assumptions of lowered status, the breaking-up of wholes or emergents, are to be distinguished from evolution—though perhaps necessary for it. The distinguishing feature of mind he finds in *enjoyment* (coupled with its obverse), *reference,* and *guidance* of conduct. In accordance with the level of evolution attained, these three attributes conspire in a degree which determines the evolutional level of mind. This reduces to the problem of discussing the antics of an amoeba in terms of enjoyment and reference, since Morgan is undecided as to evidence of *guidance* of behavior in such forms. The reviewer is frankly skeptical of the verbal worth of this method of resuscitating the pleasure-pain hypotheses; he is quite certain of its analytical worthlessness. Evolution of mind is then to be considered in terms of the extent to which prospective guidance of conduct can be recognized in the animal series, and thus freedom of choice. We are then left with the thought that this must have come in somewhere.

One arises from the perusal of this quite well-made book with renewed conviction that, however necessary as defensive or charitable pedagogy, such discussions are outside the main stream of biological development. That fossil remnants can be arranged to provide suggestive seriations of organic forms is no proof that these series are *the* routes of descent. What does emerge is a large-scale view of the world of organisms, in which the composition and pace of the mass movement are of evolutionary type and dimensions. That was settled long ago. Interest now focusses

upon the factual relations evident within organic systems and upon the properties of living matter as these may be learned about from the quantitative study of the activities and in the broad sense the behavior of organisms. Fifty years from now it may be possible to return to the evolution problem, in the light of what may be learned by then, and to discuss it with less verbalism than is possible at present. In the meanwhile, clear presentations of the views of working biologists, as given in this book, should do much to preserve for investigators a measure of safety in freedom, even in a democracy.

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Fry, T. C. *Probability and Its Engineering Uses.* New York: Van Nostrand, 1928. Pp. xiv+423.

There is increasingly widespread realization that statistical methodology is not after all so terrifying, and is becoming more necessary—at least in the sense that others are using it. Treatises on statistical method are usually either mere handbooks of formulae, of undiscussed history but with presumably mysterious powers; or else, likely to be so abstruse or obscure as to be difficult to digest. Fry's *Probability* is neither of these. It is an admirable treatise on probability, written with originality, verve, and even with a sense of humor. It is not exactly easy reading, but even a non-mathematician, such as the reviewer, is able to turn to it for guidance and comfort. The presentations are clear and orderly, and the practical uses to which the procedures advocated have been put in the working life of the Bell Telephone Laboratories give them a reality not always visible in more academic discourses on chance and reliability. A chapter on the nature, measurement, and units of "probability" establishes concrete conceptions of the term. This is followed by a discussion of permutations and combinations, including factorials, gamma function, binomial expansions. Chapter III deals with various aspects of the theory of probability and its classifiable types. Throughout, numerous examples are discussed (including the problem of card-guessing by a medium) and others are provided for working, while clever use is made of the classical paradoxes of the theory of probability. Chapters IV and V deal with the relations between probability and experiment, and the distinctions logically required by common sense are given illuminating use. Probability is not an experimental science, but experimental test none the less gives a practical basis for *approximate* evaluation of probabilities. The consequences of this view are carefully developed with regard to probability conceived as a limit. Bernoulli's theorem, Stirling's formula, and Bayes's theorem give

basis for a very suggestive discussion of the problem of sampling and experiment. Bayes's theorem is accepted as sound, *"when we can get a grip on it.* The trouble is that we so seldom can" (p. 128). It is regarded as a clue to interpreting the influence of new knowledge upon probability.

Distribution functions and continuous variables are treated in Chapter VI, averages (including expectations of a probability) in Chapter VII. Chapter VIII contains careful illustrations of the use of distribution functions met with in engineering practice. Detailed apparatus for the use of Poisson series is provided; this is followed by an exposition of the normal law, of Pearson's curves, and of Gram-Charlier series. The relations between these are clearly presented. As to the normal law, "though it can do us no harm to know how far men have been able to go in putting a foundation under an ancient monument, we shall be wise not to overlook the fact that it is, in a sense at least, venerable principally for its age. It has its uses, but it is not divinely ordained for the cure of all statistical woes. Other distribution functions which have to do with continuously varying quantities are in no better logical standing." An indication of the suggestive quality of the book is given by the section closing this chapter, in which it is pointed out that by transformation of a variable any distribution of $(x)$ may be expressed as the distribution of some function $y$ $(x)$, such that $y$ may be chosen to conform to any law we may desire. The process might be tried of treating statistical data by finding what transformation would result in a completely random distribution (i.e., equal probabilities for all intervals within a certain range).

Curve-fitting, the measure of goodness of fit, the determination of distribution functions, Sheppard's corrections, are discussed in Chapter IX. This completes the treatment of probability theory.

The primary problem of curve-fitting is taken to be that of determining how unlikely, and by how much, results from experiment are to differ from true probability, and thus of finding the relation between these two. What is needed is some method of judging the accuracy of an estimate without knowing its *inherent* probability before the experiment was performed. With the necessary information at hand, Bayes's theorem gives this. This depends not only on the experimental evidence but on all the other relevant knowledge which goes into the advance judgment. "It is a matter of cold fact that the reliance which we dare place in the result of an experiment depends upon the inherent plausibility of the result as well as upon the accuracy with which it has been obtained." A knowledge of the a priori probability is *necessary.*

Of the sections of this general field usually required in biological and psychological work—measurement of dispersion, curve-fitting, tests for goodness of fit, and correlation—all but the last mentioned are fully and delightfully discussed with a direct, consecutive quality, and with a reality

due to contact with actual data, such as is refreshing. Without discussing "statistical methods" at length, Fry gives, however, a picture of what one may expect to learn from statistical analysis, and its logical limitations, with directions to sources for details and explanations of standard terms. The brief discussion of control charts is suggestive in several ways. Chapter X devotes 65 pages to the applications of the theory of probability to detailed problems of congestion in telephone service, which indeed can supply models for other and quite different situations; Chapter XI deals with phenomena of fluctuation in physics.

Appendices provide tables of factorials, normal curve and its derivatives, the error function, Poisson series, *P*, and the like. The index might be fuller. Throughout the text, at chapter ends, adequate references are given to fundamental books and papers.

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Rabaud, E. *How Animals Find Their Way About: A Study of Distant Orientation and Place-Recognition.* (Trans. by I. H. Myers.) London: Kegan Paul; New York: Harcourt, Brace, 1928. Pp. ix+142. $2.75.

Although not so stated in this volume, this book is a more or less verbatim translation of the monograph published in 1927 at Paris (Alcan) under the title *L'Orientation Lointaine et la Reconnaissance des Lieux;* the essential ideas here involved are perhaps not to be translated with exactness as "distant orientation and place-recognition." The author is chiefly known for his treatment of large biological themes with frequent references to his specific knowledge of insects and arachnids. In other works he has discussed extensively and in rapid succession such topics as heredity and variation, reflex immobilization, adaptation and evolution, convergence, and the like, and his large treatise *Elements de Biologie Générale* is noteworthy for a certain breadth of view coupled with a nicety of statement and a desire to deal in an illuminating way with factual details—in this respect measurably distinguished from the theorizings of such French writers as Le Dantec and Bohn.

His problem in the present book has to do with the systematic experimental analysis of the orientation of progression in animals, and their exploration and examination of new and old scenes. The author very properly remarks that most of the experimentally obtained information relates to invertebrates and more especially to insects, while rigorous and well-conceived experiments with vertebrates are still for the future. Even so, one is impressed by the lack of precise experimentation as concerns the invertebrates. He deals with the usual theories of *orientation* as involved

in the *migration* of birds and considers that these two phenomena are to be kept distinct; it is not entirely clear just how this is to be done nor what is the accruing benefit to understanding. All phenomena of distance-orientation and place-recognition he believes to give evidence of sensory memory. In different cases among invertebrates diverse sensory cues appear to be significant, but Rabaud does not sympathize with those who would appeal to "unknown senses" or rely upon the hypnotic effect of pseudo-physical analogies.

One is compelled to recognize, however, that this healthy attitude is largely a declaration of faith, for, as the author says, the field is free for research if preconceived "solutions" are abandoned. This implies the desirability of quantitative experimentation, which in connection with "homing" behavior has been singularly lacking. The recent excellent work of Wolf, notable for its quantitative form and spirit, is not reviewed by Rabaud, nor is all of the older literature analyzed. The more important evidence concerning orientation of ants, due to Santschi, Cornetz, Brun, is reviewed in an interesting way, with the conclusion that in nature orientation is controlled by a complex of a number of forms of stimulation. The possibility of precise interpretation by means of experiments in which isolated modes of response are first studied separately and then in combination has not been explored.

It may to some readers be a relief to follow a treatise on *orientation* in which tropisms are not mentioned, nor Loeb's comment on wasps; but anyone with first-hand knowledge of what can be learned about tropistic behavior in ants, for example, can afford to smile. As it is, Rabaud is left with the notion that with ants, as with flying Hymenoptera, it seems that the ant "registers" its usual environment as a whole, but in such a manner that it is able on a return journey to neglect zig-zag turnings and to pursue a shortened path—a "matter of kinaesthesis and appreciation of distance"; none of which is especially illuminating.

The recorded information regarding other invertebrates is treated in a fragmentary way. The reviewer may be pardoned for pointing out that in his reference to the behavior of *Chiton,* described by the writer, Rabaud has missed the point completely, and has moreover confusedly attached some of the facts made known in a series of papers (not cited by Rabaud) on *Onchidium* to the label *Chiton,* so that the whole matter is needlessly muddled. There are other instances of hasty bolting of notes on forms outside the arthropods.

The bibliography is confessedly short; the index is a useful addition to the French version.

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ANDERSON, J. E., & GOODENOUGH, F. L. The modern babybook and child development record. New York: Norton and Parents' Mag., 1929. Pp. xii+398. $5.00 reg. ed., $10.00 de luxe ed.

BROOKS, F. D. The psychology of adolescence. Boston: Houghton Mifflin, 1929. Pp. xxiv+652.

CANNON, W. B. Bodily changes in pain, hunger, fear and rage (2nd ed., rev. and enlarged.) New York: Appleton, 1929. Pp. xvi+404. $3.00.

FOSTER, W. S., & TINKER, M. A. Experiments in psychology. New York: Holt, 1929. Pp. xvi+392. $2.25.

MCDOUGALL, W. Modern materialism and emergent evolution. New York: Van Nostrand, 1929. Pp. x+249. $2.75.

MELTZER, H., & BAILOR, E. M. Developed lessons in psychology, including objective tests with norms. New York: Harcourt, Brace, 1929. Pp. x+200.

SADLER, W. S. The mind at mischief. New York; London: Funk & Wagnalls, 1929. Pp. xvi+400. $4.00.

SONES, W. W. D., & HARRY, D. P., Jr. Sones-Harry achievement test. (Forms A & B.) Yonkers, N. Y.: World Book, 1929.

TEAD, O. Human nature and management. New York: McGraw-Hill, 1929. Pp. x+312.

THURSTONE, L. L., & CHAVE, E. J. The measurement of attitude: a psychophysical method and some experiments with a scale for measuring attitude toward the church. Chicago: Univ. Chicago Press, 1929. Pp. xii+98. $1.00.

WALLIN, J. E. W. A brief survey of special education in the public schools of Baltimore. Baltimore, Md.: Dept. Educ., 1929. Pp. 58.

———. A statistical study of the individual tests in ages VIII and IX in the Stanford-Binet scale. *Ment. Meas. Monog.*, 1929, No. 6. Pp. vii+58.

WECHSLER, I. S. The neuroses. Philadelphia: Saunders, 1929. Pp. 330. $4.00.

WHEELER, R. H. The science of psychology: an introductory study. New York: Crowell, 1929. Pp. xvii+556. $3.75.

WOODWORTH, R. S. Psychology. (Rev. ed.) New York: Holt, 1929. Pp. xiv+590.

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